

# **Scientists' Views of the Philosophy of Science**

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## Declaration

I, Hauke Riesch, confirm that the work presented in this thesis is my own. Where information has been derived from other sources, I confirm that this has been indicated in the thesis.

## Acknowledgements

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## **Abstract**

Many studies in public understanding of science emphasise that learning how to do science also involves learning about the philosophical issues surrounding the nature of science. This thesis aims to find out how scientists themselves talk and write about these philosophical topics, and how these topics get used in scientific thought. It contrasts scientists' opinions on these issues with how they are portrayed in popular science, and also contrasts them with how philosophers themselves have justified their ideas. Through analysing how scientists talk and write about philosophical topics, it aims to find out what and how scientists themselves think and learn about the nature of science, and what they would like other people to learn about it.

30 popular science books were analysed for how they treat philosophical topics on the nature of science. 40 academic scientists were then asked in a series of semi-structured interviews questions based on the philosophical topics that were found discussed most often in the books. Five philosophical topics are dealt with in detail: The demarcation question of "what is science", the philosophies of Popper and Kuhn, Occam's razor and reductionism, which reflect the most common philosophical themes in the popular science books.

In interpreting the books and the scientists' responses on these topics, I use the concepts of boundary work and boundary objects, and social identity theory. It demonstrates that philosophical topics can be used to draw boundaries and to define social identities around science or various disciplinary affiliations. Philosophies and famous philosophers like Popper also act as boundary objects facilitating scientific communication across boundaries. The talk surrounding the various philosophical categories however often hides a big variation in actual philosophical opinion, which is set slightly apart from how the philosophy itself is discussed.

# Contents

|                                                                                                                                  |    |
|----------------------------------------------------------------------------------------------------------------------------------|----|
| <b>Acknowledgements</b> .....                                                                                                    | 3  |
| <b>Abstract</b> .....                                                                                                            | 4  |
| <b>Chapter 1: Introduction</b> .....                                                                                             | 8  |
| <b>1. Outline of the thesis and background</b> .....                                                                             | 8  |
| 1.1. <i>Outline of the thesis</i> .....                                                                                          | 8  |
| 1.2. <i>Context and background</i> .....                                                                                         | 9  |
| 1.3. <i>Research questions</i> .....                                                                                             | 11 |
| <b>2. Methods</b> .....                                                                                                          | 11 |
| 2.1. <i>The pilot study</i> .....                                                                                                | 13 |
| 2.2. <i>The analysis of the popular science books</i> .....                                                                      | 13 |
| 2.3. <i>The interviews</i> .....                                                                                                 | 19 |
| <b>3. The following chapters</b> .....                                                                                           | 23 |
| <b>Chapter 2: Literature Review</b> .....                                                                                        | 28 |
| <b>1. Introduction</b> .....                                                                                                     | 28 |
| <b>2. Why teach philosophy of science? Why philosophy of science at all?</b> .....                                               | 29 |
| 2.1. <i>PUS and the teaching of scientific method</i> .....                                                                      | 30 |
| 2.2. <i>The science education community and scientific method</i> .....                                                          | 33 |
| 2.3. <i>Philosophy and science education, and the aims of philosophy of science</i><br>.....                                     | 34 |
| <b>3. Scientists' opinions on scientific method</b> .....                                                                        | 38 |
| 3.1. <i>Empirical studies by the science education community</i> .....                                                           | 38 |
| 3.2. <i>Philosophers and historians on scientists' opinions on methodology</i> .....                                             | 40 |
| 3.3. <i>Sociologists on scientists' opinions on methodology</i> .....                                                            | 43 |
| 3.4. <i>Scientists and literary cultures</i> .....                                                                               | 48 |
| <b>4. Conclusion and summary</b> .....                                                                                           | 52 |
| <b>Chapter 3: Boundaries and Identities</b> .....                                                                                | 55 |
| <b>1. Introduction</b> .....                                                                                                     | 55 |
| <b>2. Summary of the boundary approaches</b> .....                                                                               | 56 |
| 2.1. <i>Thomas Gieryn: Boundary work and the demarcation problem</i> .....                                                       | 57 |
| 2.2. <i>Boundary objects</i> .....                                                                                               | 59 |
| 2.3. <i>The influence of the boundary approaches on science and technology</i><br><i>studies</i> .....                           | 60 |
| 2.4. <i>Criticism of the boundary approach</i> .....                                                                             | 62 |
| <b>3. Summary of the social psychology approaches</b> .....                                                                      | 64 |
| 3.1. <i>Social representation theory</i> .....                                                                                   | 65 |
| 3.2. <i>Social identity theory</i> .....                                                                                         | 67 |
| 3.3. <i>Criticisms of the social psychology approaches</i> .....                                                                 | 70 |
| <b>4. Social identity frameworks applied in STS</b> .....                                                                        | 72 |
| 4.1. <i>Contrasts and similarities between boundary work and social identity</i> ...                                             | 72 |
| 4.2. <i>What a social identity perspective can add to the boundary work theme</i><br><i>(and the possible limitations)</i> ..... | 75 |
| <b>Chapter 4: What is and isn't science</b> .....                                                                                | 79 |
| <b>1. Introduction</b> .....                                                                                                     | 79 |
| <b>2. Philosophical introduction</b> .....                                                                                       | 81 |
| 2.1. <i>Induction</i> .....                                                                                                      | 81 |
| 2.2. <i>Hypothetico-deductivism: logical positivism and falsificationism</i> .....                                               | 83 |

|                                                                                                          |     |
|----------------------------------------------------------------------------------------------------------|-----|
| 2.3. <i>Social norms and the social side of science</i> .....                                            | 85  |
| 2.4. <i>The development of philosophy of science in France</i> .....                                     | 86  |
| <b>3. Science defined in the books</b> .....                                                             | 87  |
| 3.1. <i>Philosophical asides</i> .....                                                                   | 88  |
| 3.2. <i>The different implicit philosophies</i> .....                                                    | 91  |
| <b>4. Comments on what is science in the interviews</b> .....                                            | 101 |
| 4.1. <i>Initial reactions</i> .....                                                                      | 102 |
| 4.2. <i>Attitudes towards philosophy, and education received on scientific method</i><br>.....           | 103 |
| 4.3. <i>The philosophical opinions</i> .....                                                             | 106 |
| <b>5. Conclusions and summary</b> .....                                                                  | 120 |
| 5.1. <i>Boundaries and repertoires</i> .....                                                             | 121 |
| 5.2. <i>Three types of opinions on science and scientific method</i> .....                               | 123 |
| 5.3. <i>French and English conceptions of science</i> .....                                              | 124 |
| 5.4. <i>Summary</i> .....                                                                                | 127 |
| <b>Chapter 5: Popper and Kuhn</b> .....                                                                  | 129 |
| 1. <b>Introduction</b> .....                                                                             | 129 |
| 2. <b>Philosophical introduction</b> .....                                                               | 130 |
| 2.1. <i>Popper</i> .....                                                                                 | 131 |
| 2.2. <i>Kuhn</i> .....                                                                                   | 133 |
| 3. <b>Popper and Kuhn in the popular science books</b> .....                                             | 135 |
| 3.1. <i>Popper: The philosopher as an authority figure</i> .....                                         | 136 |
| 3.2. <i>Critical discussions of Popper</i> .....                                                         | 140 |
| 3.3. <i>Kuhn: The historian of science legitimising the outsiders?</i> .....                             | 142 |
| 4. <b>Popper and Kuhn in the interviews</b> .....                                                        | 145 |
| 4.1. <i>The celebrity of Karl Popper</i> .....                                                           | 146 |
| 4.2. <i>Falsification and the logic of science</i> .....                                                 | 149 |
| 4.3. <i>The relative obscurity of Kuhn</i> .....                                                         | 157 |
| 4.4. <i>Revolutions, paradigms, and the search for truth</i> .....                                       | 159 |
| 5. <b>Discussion and summary</b> .....                                                                   | 167 |
| 5.1. <i>Popper and Kuhn's ideas in popular science: Philosophers as authorities</i><br>.....             | 167 |
| 5.2. <i>Reputations predisposing reaction, and philosophers as boundary objects</i><br>.....             | 172 |
| 5.3. <i>Summary</i> .....                                                                                | 174 |
| <b>Chapter 6: Occam's razor and simplicity</b> .....                                                     | 176 |
| 1. <b>Introduction</b> .....                                                                             | 176 |
| 2. <b>Philosophical introduction to Occam's razor</b> .....                                              | 178 |
| 2.1. <i>Usual philosophical introductions to Occam's razor</i> .....                                     | 178 |
| 2.2. <i>What is simplicity?</i> .....                                                                    | 181 |
| 2.4. <i>Three types of Occam's razor</i> .....                                                           | 182 |
| 3. <b>Occam's razor in the popular science books</b> .....                                               | 184 |
| 3.1. <i>Occam's razor as an authoritative rule of distinguishing science from non-<br/>science</i> ..... | 185 |
| 3.2. <i>Simplicity, elegance and aesthetic as a feature of good science and the<br/>world</i> .....      | 188 |
| 3.3. <i>Dissenting voices</i> .....                                                                      | 191 |
| 3.4. <i>Discussion of the uses of simplicity in the books</i> .....                                      | 195 |
| 4. <b>Occam's razor in the interviews</b> .....                                                          | 196 |

|                                                                                                                                          |            |
|------------------------------------------------------------------------------------------------------------------------------------------|------------|
| 4.1. The ontological razor and intuitive evaluations of simplicity.....                                                                  | 197        |
| 4.2. Explanations for simplicity and the epistemological razor.....                                                                      | 200        |
| 4.3. Criticisms of Occam's razor: The merely methodological razor and outright rejections .....                                          | 203        |
| <b>5. Discussion and summary .....</b>                                                                                                   | <b>205</b> |
| 5.1. Occam's razor vs the principle of simplicity.....                                                                                   | 205        |
| 5.2. Relevance of simplicity for science .....                                                                                           | 207        |
| 5.3. Implications for philosophy.....                                                                                                    | 208        |
| 5.4. Summary.....                                                                                                                        | 211        |
| <b>Chapter 7: Reductionism.....</b>                                                                                                      | <b>212</b> |
| 1. Introduction .....                                                                                                                    | 212        |
| 2. Philosophical introduction.....                                                                                                       | 216        |
| 3. Reductionism in the popular science books .....                                                                                       | 218        |
| 3.1. The life sciences .....                                                                                                             | 219        |
| 3.2. Situating reductionism within the Nature/Nurture debate .....                                                                       | 224        |
| 3.3. The physical sciences .....                                                                                                         | 233        |
| 4. Reductionism in the interviews .....                                                                                                  | 235        |
| 4.1. Scientists' own definitions of reductionism.....                                                                                    | 236        |
| 4.2. My exposition of reductionism and reactions towards it .....                                                                        | 242        |
| 5. Discussion and summary .....                                                                                                          | 252        |
| 5.1. Reductionism as a philosophical identity.....                                                                                       | 253        |
| 5.2. The value of reductionism for the disciplines and its implications for the development of philosophical debate within science ..... | 257        |
| 5.3. Implications for popular science.....                                                                                               | 259        |
| 5.3. Summary.....                                                                                                                        | 261        |
| <b>Chapter 8: Conclusions.....</b>                                                                                                       | <b>263</b> |
| 1. Introduction .....                                                                                                                    | 263        |
| 2. Main themes.....                                                                                                                      | 265        |
| 2.1. Philosophical authority and the representations of philosophies.....                                                                | 265        |
| 2.2. Boundary work and philosophical identities.....                                                                                     | 271        |
| 3. Lessons.....                                                                                                                          | 275        |
| 3.1. Lessons for social study of science.....                                                                                            | 275        |
| 3.2. Lessons for PUS and science education.....                                                                                          | 278        |
| 3.3. Lessons for philosophy.....                                                                                                         | 279        |
| 4. Concluding comments and future directions.....                                                                                        | 280        |
| <b>Appendix I: List of respondents.....</b>                                                                                              | <b>283</b> |
| <b>Appendix II: List of the popular science books.....</b>                                                                               | <b>286</b> |
| <b>Appendix III: Table of the popular science books ordered by subject .....</b>                                                         | <b>290</b> |
| <b>Appendix IV: Initial letter .....</b>                                                                                                 | <b>292</b> |
| <b>Appendix V: Initial letter (French).....</b>                                                                                          | <b>293</b> |
| <b>Appendix VI: Interview schedule .....</b>                                                                                             | <b>295</b> |
| <b>Appendix VII: Coding frame, interviews.....</b>                                                                                       | <b>296</b> |
| <b>References .....</b>                                                                                                                  | <b>305</b> |

## Figures:

|                                                                             |     |
|-----------------------------------------------------------------------------|-----|
| Figure 1: Three possible structures of benzene - which one is simpler?..... | 202 |
|-----------------------------------------------------------------------------|-----|

# Chapter 1: Introduction

|                                                       |    |
|-------------------------------------------------------|----|
| <b>1. Outline of the thesis and background</b>        | 8  |
| 1.1. <i>Outline of the thesis</i>                     | 8  |
| 1.2. <i>Context and background</i>                    | 9  |
| 1.3. <i>Research questions</i>                        | 11 |
| <b>2. Methods</b>                                     | 11 |
| 2.1. <i>The pilot study</i>                           | 13 |
| 2.2. <i>The analysis of the popular science books</i> | 13 |
| 2.3. <i>The interviews</i>                            | 19 |
| <b>3. The following chapters</b>                      | 23 |

## 1. Outline of the thesis and background

### 1.1. *Outline of the thesis*

This thesis will examine scientists' opinions on philosophical topics on the nature of science, and compare how popular science authors represent their views in the public sphere when they need to communicate science, with how scientists in general represent their views on those topics when asked in anonymous interviews. I first looked at a selection of recently published and critically acclaimed popular science books, drawing out the representations of what is science, as well as some of the most frequently mentioned and discussed philosophical topics. These topics were then put to a selection of scientists in a series of semi-structured interviews. An extra dimension this thesis explores is whether there is any striking difference between UK based scientists and scientists based in France, because philosophy of science has developed in a different direction in France, and whether this fact is reflected in the philosophical opinions of scientists there.

The thesis will contrast the views that scientists have on the various philosophical topics within the two studies (the books and the interviews) and with philosophers' thoughts on them. It will look at how the philosophical themes are used in scientific discourse, and what possible roles philosophical concepts can play within it. The thesis will therefore also look at philosophical themes in scientists' writing and discourse as boundary work and as a rhetorical drawing of a philosophical and disciplinary identity.

## 1.2. Context and background

Science communicators and public understanding of science (PUS) researchers have regularly troubled themselves by trying to assess the public's "scientific literacy". Miller (1987), for example, conducted surveys of the public's understanding of science for the (US) National Science Foundation. It used to be often assumed that somebody is scientifically literate if he/she knows a lot of facts about science, Miller's contribution however was to argue that if people knew about how science works, they would be able to assess *new* pieces of scientific knowledge thereby being able to contribute better to the public debates.

Determining the distinguishing features of the scientific method is a contentious philosophical question. Many PUS researchers, as well as plenty of scientists that engage in science communication, have a pet philosophy which they assume is shared by scientists generally. Miller's program has come into question: Bauer and Schoon (1993) for example argued that Miller's conception of science in his evaluations of literacy is too simplistic and adheres too much to a particular philosopher (in this case Popper). Durant (1993) even argued that teaching "how science works" should be supplemented by teaching "how science *really* works". Neither criticism replaces Miller's criteria of how science works with an alternative, Durant's version of how science really works is more of a commentary that scientists are not as objectively rational as they portray themselves. But more generally, PUS research has not replaced it with any better idea of what the method of science is because the discipline has largely moved on from the idea of scientific literacy altogether, and they now concentrate on other things.

But the issue has not vanished. Many scientists (for example those based in universities) themselves have an interest in teaching people to understand scientific method, because they are training the next generation of scientists. Likewise, school curriculum developers need to know what to teach about scientific method (in this area the literature is large and very developed, for example Matthews 1994, Osborne et. al 2003). Much research in this area has struggled with the fact that there is no consensus emerging from philosophy of science that could inform the curriculum. For them it should be sensible to have a look at what scientists themselves think and teach about scientific method. There is a good reason for PUS researchers to be going back to the scientist's opinion. Miller, in his answer to

Bauer and Schoon argues that his idea of scientific method is just that of ordinary scientists (Miller 1993). This is however presupposing that we know what ordinary scientists really do think on scientific method.

That philosophy and philosophical notions on the nature of science play a large role in the communication of science is easily seen by an informal glance at popular science books, where the authors are more free to discuss the reasoning and working behind the science away from the strict requirement of the technical literature. As some previous studies have shown, such as Turney (2001) or Nieman (2000), the range of philosophical opinions in popular science can be quite varied. At the same time, there is an often voiced expectation that scientists either do not care about philosophy, or that they are all Popperians (these are the two typical responses I get when describing my research, from most philosophers and historians of science, and some scientists themselves). With this common expectation of scientists' philosophies in mind, this thesis aims to find out scientists' views on philosophy, and how philosophy is used when communicating science, and when talking more informally about science.

It is also perceived by some French philosophers that French people and scientists have a more generally positive attitude towards philosophy of science (more usually called "epistemology" in France), through the influence of traditional French philosophers of science, Bachelard and Canguilhem (see chapter 4): "In France, epistemology is very *agreeable* to scientists and many scientists *dream* to be considered as epistemologists" (Bruno Latour, quoted in Callebaut 1993, p.106, original emphasis). An empirical estimation on Latour's claim is also one of the aims of my study, and will be further discussed in chapter 4.

This thesis is also intended as an argument for using the sociological study of the philosophy of scientists for the help of the philosophy of science itself, and for making it relevant to how science is understood – by the public as well as ultimately scientists themselves. My own background, with degrees in physics as well as philosophy of science, has led me to become concerned about the contribution philosophy can make to the (public as well as scientific) understanding of science and its conceptual foundations and difficulties. This thesis will show how the two disciplines of sociology and philosophy of science can complement each other by

using a science and technology studies approach to the philosophy of science by scientists.

### *1.3. Research questions*

In this thesis I will be addressing the following research questions:

- *How do scientists view the philosophical topics on the nature of science?*
- *How do these topics get used in scientists' discourse?*
- *How do these topics shape scientists' thinking about science?*

I will also be looking at how the different situations of scientists' discourse on philosophy compare, in particular I will be looking at:

- *What are the differences in opinion and use of philosophy between the books and the interviews?*
- *What are the differences (in the interviews) between UK and France based scientists?*

Finally, I believe that my study can make some important points not just for the social study of science, and PUS, but also for philosophy of science.

- *What relevance does the study have for social studies of science, the philosophy of science and the fields of public understanding of science and science education?*

## **2. Methods**

The research has followed a broadly iterative method. First I performed a pilot study of interviews with four members of a UK based university physics department, where more topics were covered, but in less depth than in the main study. Afterwards 30 popular science books were analysed, where I got an overall idea of the philosophical ideas that get communicated in popular science, which compared quite well with the topics that had been discussed in the pilot interviews. Finally a further 36 interviews were conducted with university scientists, 23 scientists based



in the UK and 13 in France, focussing on the most frequent and interesting topics in the books.

Through the decision to perform a qualitative study, this thesis is not intended to show a representative sample of scientists' opinions (although I have tried to construct my sample as representatively as possible), but rather to give a perspective of the different opinions that exist within science regarding the nature of science. One of the ideas at the beginning was to supplement the study with a quantitative survey, but it soon became apparent that especially abstract topics like those of philosophy can be interpreted so widely that simply counting the number of "Popperians" will not give a worthwhile insight into the thinking of working scientists. The pitfalls of such a survey are well demonstrated by Crease's (2001 and 2002) admittedly light hearted and informal sampling of physicists' opinions on realism which gave us the phenomenal result that 7% of physicists believed atoms were not real, and 3% even thought the earth did not exist. This does not tell us the scientists' interpretations of science. Are these 3% of scientists idealists? How do they justify that opinion?

The analogy that Bauer et al. give to characterise the difference between qualitative and quantitative research is:

If one wants to know the colour distribution in a field of flowers, one first needs to establish the set of colours that are in the field; then one can start counting the flowers of a particular colour. (Bauer, Gaskell and Allum 2000 p.8)

This thesis is in a way an attempt to find out the different categories in which scientists talk and write about the nature of science. Generally, in the chapters below, when referring to the numbers of books, or interviewees who responded a certain way I will use qualitative terminology such as "some", "a few" or "most". Only in limited circumstances I felt that giving the precise numbers is appropriate. Thus in chapter 5 will give a number for how many books have named Popper, as in that case the number is indisputable. However, very often, for example when it came to assessing how many scientists *agreed* with Popper, there are many

borderline and indeterminate cases so that giving precise numbers I felt would be misleading (see also Hammersley 1996).

### *2.1. The pilot study*

Four members of a physics and astronomy department in the UK were interviewed on their opinions of scientific method and philosophy. The interviews were held to a fairly tight schedule of questions, but general discussions on the nature of science could easily extend over the whole length of the interview. Since I had at first only vague ideas about what kind of themes the scientists would find interesting to talk about, and which philosophers they were likely to know by name, a lot of ground was covered in the pilot study which then helped to identify themes that were interesting and worth investigating further.

The pilot interviews have been transcribed and read through before the study of the popular science books was started, and they have been coded with qualitative data analysis software ATLAS.ti at the same time as the main interviews. In this way the results of the pilot study were included in the main analysis. The coding is not quite as dense in the pilot interviews as it is in the main study, because many of the topics discussed in the pilot were not investigated further, while there were some topics that have been included in the main study which were not discussed in all the pilot interviews. Nevertheless, the pilot interviews brought out interesting as well as representative ideas, and they will therefore feature in the thesis alongside the main study. Quotes taken from the pilot study will be clearly marked as they will have the respondent numbers 1 to 4 (see Appendix I for a list of interviewees).

Participants for the pilot study were contacted through the help of a senior member of the department who introduced me to another, junior, researcher, who in turn introduced me to two of her colleagues.

### *2.2. The analysis of the popular science books*

The popular science books were chosen so that the sample consisted of books that are both demonstrably influential as well as written by scientists themselves. Because of the nature of the research questions, I wanted to have a sample of how scientists themselves would like to have science, and the nature of science, presented to intelligent laypeople. Since there are plenty of popular science books

that have been written by journalists or even authors who specialise in writing popular science, the decision was made to concentrate on scientists' own popularisation efforts. A comparison to how non-scientists write popular science would have been very interesting, and I have seriously entertained that idea for a while before deciding that it would stretch the main research question of "*scientists'* views of the nature of science" too much.

I followed a qualitative data analysis method for the books which allowed me as much as possible to make comparisons with the interviews. After reading a book I scanned the pages where the author was talking about a philosophical topic, where I have tried to be as broad with my interpretation of philosophical topic as possible. The pages were analysed with qualitative data analysis software, which in the first instance was NVivo but was later switched to ATLAS.ti.

The books considered were selected from the shortlists of the "Aventis prize" (called the "Rhône-Poulenc prize" until 1999, and since 2006 the "Royal Society prize") for popular science books between 1998 and 2004. As the thesis is about the popular discourse on philosophy by scientists themselves, only authors were included who have some first-hand experience of science themselves; books about mathematics were also filtered out<sup>1</sup> (Royal Society 2008, see appendix II for a full list of the books that were chosen). Ultimately, the threshold on who to include was set fairly low, so that authors like Matt Ridley were included, who is no longer a practising scientist and has been writing popular science professionally for a long time now.

The prize shortlists were chosen to provide a list of books written by respected authors. This has been done due to the absence of bestseller lists for popular science and the fact that even books of doubtful scientific merit are routinely classed as popular science (Myers 2003). Who to count as a scientist is a rather arbitrary distinction, but I settled on authors who either hold a doctorate in a science discipline, or have at least published in peer-reviewed science journals. In what counts as a science the decision was to follow the judgement of the prize judges in constructing the shortlist. Therefore, while mathematics was not included, writers

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<sup>1</sup> It is usually held that due to its lack of empirical evidence, mathematics faces a different set of philosophical problems to the empirical sciences (see Giaquinto 2002)

such as Steve Pinker were, because he describes himself as a neuroscientist in a psychology department, and his first-hand experience of science is by anyone's measure more scientific than that of, say, Bill Bryson whose book (Bryson 2003) was discounted from the sample despite having received glowing reviews from scientists.<sup>2</sup>

I have also had a brief look at popular science books in France, although I have decided not to include them in this study: In the absence of a similar criterion for indigenous popular science books in France I had difficulties deciding on which books to sample. Here there were the same potential pitfalls as in the UK, because I have seen books of very dubious scientific merit sold alongside other popular science books. Another problem was that, even though there were a lot of French popular science books on sale, most were translations from the more internationally focussed books written in English, including many from my sample (see also the discussion in chapter 4, section 5.3).

Throughout this thesis I have striven to use a gender neutral tone when referring to an unspecified person. When referring to an unspecified popular science author from the sample I have however used the male form, as all the popular science authors in the sample were male.

### *Overview of the books*

Of the 30 books I examined six on physics, nineteen on the biomedical sciences and two on computer science. A further three books are mostly interdisciplinary. Of the physics books, five (Davies 1995, Deutsch 1997, Greene 2000, Hawking 2001, Kirshner 2002) are on fundamental physics and cosmology (with Deutsch devoting considerable space to philosophy of science), and one (Buchanan 2002) is on networks and the "small world" phenomenon. Of the books on the biomedical sciences, five (Dormandy 1999, Gigerenzer 2002, Horrobin 2001, Weinberg 1998, Wolpert 1999) are concerned with medical topics, eleven (Dawkins 1995 and 1997,

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<sup>2</sup> There is an inevitable vagueness about the working definition of science for the purposes of this thesis, because the question "what is science" is itself something that was put to the interviewees to define. At the end, the proper definition of what science is, is not generally agreed on (see chapter 4), and therefore this thesis has started by proceeding from the boundaries set by other people (the Aventis prize judges) and then settling on three undisputed and "basic" sciences of physics, chemistry and biology for the interviews (see below). But even then there were cases where the

Jones 1997, Fortey 2000, Leroi 2003, Pinker 1998 and 2002, Mark Ridley 2000, Matt Ridley 1996 and 2003, Wills 1998) are on evolution and/or genetics, two (Mayr 1997, Wilson 1998) are general descriptions of biology and the philosophy of science, and one book (Sapolsky 2001) is an autobiographical account of fieldwork in primatology. A further book (Grand 2003) is possibly to be classified as either life science or computer science because it is an account of using computer science to model evolution. The only other books on computer science are Naughton's 1999 history of the internet, and possibly Buchanan's book on networks which was counted as physics above. Three books (Diamond 1997, McManus 2002, Webb 2002) are about topics that are being looked at from the angle of several different sciences, where Webb's book concentrates more on physics and astronomy and the other two more on the biological, and even the social sciences. (A table of the books is added as appendix II).

This overview of the sample is not to be understood as a classification, though there will be a rough division between life and physical sciences for the purposes of analysing comments on reductionism in chapter 7. Many books fall on the borders between some of my categories or into several of them. Gigerenzer's book for example is primarily on the use and understanding of statistics and therefore more a comment on science in general, even though his examples draw almost exclusively from medicine.

Whichever way the doubtful cases are classified, the life sciences outnumber the physical sciences by a large margin. Within the life sciences medical and evolutionary topics dominate, while the physics books are almost exclusively about cosmology and/or quantum physics. This spread reflects the wider publishing trends among popular science (see Turney 2007 for some thoughts on current trends in popular science).

Philosophically, there is a large variation in the interests and representations of scientific method, and of explicitly mentioned philosophical topics. A few books (Deutsch 1997, Mayr 1997, Wilson 1998 and Davies 1995) cover philosophical topics and the nature of science extensively. Deutsch provides a slightly modified

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actual disciplinary status (and identity) was not as clear as the departmental affiliation suggests, as scientists often move in and out of their disciplines.

Popperian view on scientific method and comments on a range of other philosophers and philosophical notions, all while also explaining fundamental physics. Mayr's book is about the foundations (and the history) of biology, and as such also touches on most major philosophers and philosophies. Wilson's book is in this respect slightly different from the other two, as it is more about Wilson working out his own philosophy of science, which is a special version of the unity of the sciences (including even the arts and humanities), which he calls "consilience". Davies' book deals substantively with a scientific problem that borders on the philosophical, the nature of time, and it is therefore also very philosophical in nature. It is set apart from the others because it deals more with philosophy of physics and of time in particular, rather than general philosophy of science or epistemology. The book is therefore unusual because it discusses relatively obscure philosophers such as Herbert Dingle and J. Smart, but makes only a glancing reference to Popper.

At the other extreme, Sapolsky's book on his life studying baboons in Kenya has very little on the process of actual science, and instead recites the life of a field scientist, complete with long chapters about travelling and living in Africa. Horrobin's book about the evolutionary causes of madness is also very autobiographical about his life in science in places, though it features plenty of scientific detail lacking in Sapolsky. It describes the author's own theories on schizophrenia rather than popularising science that has already been generally accepted, and is in this respect quite atypical of popular science books<sup>3</sup>. Wolpert's book is also very autobiographical, describing the author's own brush with clinical depression, and this book marks his efforts as an academic in an adjacent but unrelated field to understand it, and share the information. Though this particular book does not touch upon scientific method or philosophy in any great detail, Wolpert's views on these topics were expressed clearly enough in his earlier popular science book *The Unnatural Nature of Science* (Wolpert 1992, see chapter 2 on Wolpert's role in the science wars).

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<sup>3</sup> Horrobin's science has been and continues to be very controversial. The *British Medical Journal* published a very scathing obituary (Richmond 2003), which caused a very heated debate on Horrobin and his science (see Davies 2003 for a summary of the "rapid responses" for the obituary)

Also short on describing actual scientific method are several books on the history of science. Robert Weinberg's book is a short history of the process that led to the discoveries of the mechanisms of cancer, Dormand a rather longer and at times rambling history of the discovery and treatment of tuberculosis over the centuries. As is probably unsurprising of histories written by a medic and a medical scientist, both these books have a whiggish feel to them, which reaches almost satirical levels in Dormand. Both accounts are relatively sparse on scientific method or philosophy. Another historical book, Naughton's history of the development of the internet, however does include some substantial comments on the philosophy of Thomas Kuhn, as does Grand (2003), which is an account of how the author developed computer programs that aim to mimic the properties of life and evolution.

The rest range from heavily philosophically orientated because the subject matter is somewhat intangible, like the books on cosmology by Kirshner and Greene (and of course Davies, mentioned above) but without any heavy emphasis on scientific method, to more episode-by-episode accounts of scientific discoveries or controversies that emphasise the detail of the science, but leave philosophical and methodological issues largely aside. Some of those books, such as those on evolution, do on occasion contain interesting comments on some topics, such as reductionism or Occam's razor, and therefore will feature in the relevant chapters quite heavily. Finally, Gigerenzer's book stands out as a defence of using Bayesianism for the public understanding of statistics (see chapter 4, sect. 2). Whether that also translates into an endorsement of Bayesianism as a general philosophy of science is however quite unclear, although Gigerenzer is unconvinced by the subjective interpretation of probability, on which most modern forms of methodological Bayesianism rest (see Gillies 2000).

Even though the life sciences books hugely outnumber the physics books, in the analysis below almost any of the topics I will discuss will feature about as many books from physics as from the life sciences. This means that for the physics authors, on average, philosophical topics or musings were much more important, and that deserves to be pointed out. Before jumping into any conclusions though about how much more important philosophy is for physics than it is for the life sciences, it also has to be considered that it is the type of topics that lend themselves

for popularisation that determines what gets written about. More fundamental disciplines like cosmology and evolution almost invariably feature philosophical topics, whereas more technical topics such as medicine or genetics rarely do. Therefore the real difference in philosophical tone is not between physics versus the life sciences, but between theoretical/fundamental science versus applied science.

### *2.3. The interviews*

The interviewees were chosen in two different ways. In the UK, I have listed all university departments of Physics/Astronomy, Chemistry and Biology that received a RAE score of over 4 in 2001 (HERO 2002). This was done to ensure that the departments were research intensive rather than focussing on teaching. That requirement proved to be somewhat superfluous at the end, as most “straight science” departments of the type that I was interested in were situated in the old, research intensive universities anyway, while the science teaching in teaching universities is usually concentrated in applied and interdisciplinary sciences.

Within these departments I visited the website and contacted a purposively selected member of staff. Since the aim was to achieve a roughly representative mix of genders and a roughly even split of junior, mid- and late career researchers, the scientists that were selected from the departmental staff list were usually chosen as to suit the requirements at the time. Usually a group of 20-30 scientists were contacted at a time, and because it took a while for the answers to come back in, it was difficult to achieve the desired mix. In several cases the scientist contacted introduced me to colleagues who were interested to talk to me as well. Generally the aim was to go to as many different places as possible to get a diverse sample from around England. There was one further respondent who had to cancel the day before the interview. He did however write a detailed email in response to the initial letter, which addressed several relevant issues even though I did not get to ask him any questions directly. That email was treated as one of the interviews during the coding.

In general I only contacted scientists with at least a PhD. On three occasions when one of the scientists was kind enough to ask their colleagues to help me as well, I also interviewed PhD students, two of those during the pilot study. Even though the PhD is an accepted standard with which scientific merit is measured,



there are many people who help in the production of science who do not hold a PhD and are not working towards one, such as lab technicians. At the end, just as was the case with judging the book authors and with judging which discipline counted as science, the choice ultimately had to be somewhat arbitrary.

About a third of the interviews (13) were held in France. Contacting French scientists required a slightly different strategy, as I found the websites of French universities and institutions of higher education generally very hard to navigate by department. However, research groups and laboratories usually maintain their own websites, and the scientists were therefore chosen from among these. Though I found no comprehensive list of research laboratories in France, there was a site that lists all laboratories that are funded, or co-funded by the national research funding council (CNRS 2008), which also lists the laboratories according to the disciplinary affiliation of the university departments in which the laboratories are situated. Therefore a list of all CNRS sponsored laboratories in Paris was constructed, which incidentally also acted as the “quality control” that ensured, like the RAE in the UK, that only research active scientists were selected. From that website I selected some laboratories and wrote to the director and asked whether they would mind being interviewed, or if they could pass the request on to their colleagues. However even though the respondents were happy to be interviewed themselves, most did not find any colleagues for me to interview, so the selection included proportionally many senior scientists, and also unfortunately not many chemists. The French interviews were restricted to Paris, as there was only a limited time and budget in which to hold them. The interviewees were given the choice of talking in English or French. Quotes from a French interview below in the thesis will be presented through my own translation, with the original in a footnote<sup>4</sup>.

The sample included three scientists whose career included working and living in both French and English speaking cultures. One was a UK scientist who has earned his PhD in Francophone Switzerland, one was a French postdoc in the UK, and a third was a Paris-based scientist from an Anglophone country. Also, since this thesis aims to compare how popular science represents science with how

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<sup>4</sup> I am grateful to Bérangère Bacquey for checking the translations for accuracy.

“normal” scientists represent science, it is interesting to note that four of the scientists were popular science book authors themselves: One in the UK, and two in France – I have read their books to see if they could supplement the interviews (at the end they did not add much of value to the interview, so they have been left out of the study due to the complication citing them would make to the anonymity of the respondents). Another scientist has subsequently co-authored a popular science book.

The initial letter described that I was a research student who wanted to find out how scientists teach and think about scientific method and the nature of science, and gave the reason that it is an important exercise for informing science communication (for a typical copy of an initial approach letter see Appendix III (English) and Appendix IV (French)). The letter has been carefully worded so as not to include the word “philosophy” itself, because I thought it might conceivably prejudice the potential interviewees. I then followed up the letters by emails to those that had not responded after a fortnight. In both letters and follow-up emails I invited the respondents to ask me for further details about my research, which however only one respondent took advantage of, who I sent a copy of my original research proposal.

Of the scientists initially contacted, around a quarter have responded positively. Although the sample was intended to be very broad, a small selection bias towards scientists well disposed towards philosophy in general could not be avoided. However, while nobody in the interviews was openly hostile to philosophy, several scientists were indifferent to it, while others believed that philosophy, while maybe worthwhile in its own right has not much to contribute to science. There were also some scientists who responded to the request not because they were particularly interested in the questions, but because they always try to help in these sort of requests, or because they were simply curious. Nevertheless, this probable selection bias prevents overall conclusions from being made about the number and representativeness of attitudes towards philosophy as well as knowledge of it. It does however present a rather broad and diverse range of opinions, and therefore this study will help to establish what the different colours of the flowers are rather than their distribution, in Bauer et al’s (2000) metaphor quoted above.

The interviews were usually held in the respondents' offices. Most of the exceptions were the postdocs and PhD students who usually did not have their own offices, and we often had to relocate to departmental common rooms, the supervisor's office, the laboratory, and in one case over lunch in a pub. The interviews all lasted between half an hour and one and a half hours, although the vast majority lasted around one hour, which is the amount of time asked for in the initial correspondence.

The questions that were asked were first of all about the respondents' research so that the later statements about scientific methods could be put into the context of their own work, but it also put the scientists at ease by letting them talk about their own subject and establish a rapport with the interviewer. They were then asked what they thought was science and what they thought distinguished science from things they think are not science. Often the answer involved the "scientific method", in which case I asked what that was. Then the scientists would be asked how they learned about science and the scientific method themselves, for example, did their opinion derive from reading philosophy, going to lectures or copying their supervisor. That question would be followed up by asking about how they approach (or would approach, in the case of the non-teaching scientists) that subject with their own students. Then there was a discussion on four particular philosophical topics: the philosophy of Popper, that of Kuhn, the concept of reductionism and the value of simplicity, or Occam's razor. These were all topics that in the pilot study and the books were some of the most discussed philosophical ideas. These topics, and the question of what is science, would usually suffice to fill out a lively discussion with the respondents.

One complicating aspect of the interviews was that often the respondents were considering a philosophy seriously (or even at all) for the first time, which means often the initial reaction towards a topic was slightly different to the more considered attitude developed over the subsequent discussion. This shows that the interview study would have obtained slightly different results than a questionnaire on whether people liked Popper, or falsificationism. However this complication also pointed out that many of the opinions encountered were not final and that asking the same people the same questions now would get different results than the first study has. As one respondent put it, the way I explained the conflicting philosophies of

Popper and Kuhn they both sounded reasonable, and that he would need some time to mull it all over before he commits to an answer (38 Senior, Physics, Male, France)<sup>5</sup>.

This effect will be addressed by presenting the results from the interviews first as “initial reactions” to my question, followed by a more detailed analysis of the following discussions. Because there were of course also many respondents who did not change their viewpoints, as well as respondents who had already made their position clear on a subject in the initial phases of the interview, there is some considerable overlap between the initial reactions sections and the rest of the chapter’s discussion. This way of presenting the results demonstrates not only that scientists’ philosophical opinions are always subject to further introspection, but it also highlights some interesting differences between initial reaction and considered opinion to some philosophical topics.

The interviews were transcribed fully, including all “erms” and “ahs” and repetitions, although the quotes that I will present in this thesis will be cleaned up of these, unless I thought that would alter the feel or meaning of the quote. I have not altered the sentences in any other way, even if sometimes the grammar of the spoken conversation can render the quote a little hard to read. The interviews were then coded in ATLAS.ti.

### **3. The following chapters**

In the next chapter I review some of the literature on the topic of scientists’ views of the philosophical topics of the nature of science. The literature and previous similar studies are very diverse in methods and aims, as well covering such a varying range of philosophical topics that any comparisons will be difficult. I nevertheless identified some studies that I make use of in my thesis, including both qualitative interviews with scientists and analyses of popular science books. The literature review chapter is organised in two parts. In the first I review different literatures on why the subject of philosophy in the public understanding of science and science education should be seen as important, and there the literature ranges from studies

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<sup>5</sup> References to the interviews will be marked by a number unique to each respondent, followed by their seniority, discipline, gender and country of residence. See Appendix I for a full list.

of teaching the nature of science in school to it being one of the fundamental purposes of philosophy of science itself – because what good is philosophy of science if scientists never learn about it? Here I am touching on a larger debate going on within philosophy of science. The second section reviews previous studies that have looked at scientists' opinions of philosophical issues, and ends with a small history of the relationship between scientists and philosophy which will set some of the background to the topic of this thesis.

In chapter 3 I review the theoretical strands with which I hope to analyse the scientists' opinions. I focus mainly on two traditions which I argue are conceptually related and which I am both using throughout the thesis to interpret the scientists I read and talked to. On one side are Gieryn's (1999) work on social boundaries and Star and Griesemer's (1989) concept of boundary objects, which have become a staple theoretical background in science and technology studies, especially concerning scientists' own opinions on their activities. The other side are social identity theory and the concept of social representations taken over from social psychology, where it has been enormously successful and has developed a large background of studies and research. Gieryn's boundary approach has only rarely been used outside of science and technology studies, while unbelievably I seem to be among the first to use social identity theory within science and technology studies. I point to the similarities and the differences of the two approaches and also suggest where they could possibly intersect and complement each other.

Chapters 4 to 7 show the results from my study and develop the categories of interpretation with which I hope to make sense of it. I have striven to give these chapters a comparable structure. After introducing each chapter's topic I will include a brief introduction to the relevant philosophical debates, then I show the results from the popular science books then the results from the interviews. Putting the popular science books first reflects the chronological order in which I have conducted the study.

Chapter 4 reviews the implicit philosophies of the scientists; what the book authors have written about what is science, and the interviewees' answers to that question. I present my efforts to find the different ideas that people have had regarding the general nature of science, and what sets it apart from other activities. Chapter 4 is different from the three other ones because I do not look at any

particular philosophical topic and therefore I will not analyse the different ways the topic has been used and interpreted. This chapter in a way sets the scene which draws attention to the different ways scientists have responded to the nature of science. I hope however that the chapter can stand alone, despite the relative lack of theoretical analysis compared to the others, and that it will show the different categories in which science is seen, mainly I think subdivided into three major philosophical strands (inductivism, hypothetico-deductivism and the social side of science).

Chapter 5 presents the thoughts the writers and respondents had on the philosophers Popper and Kuhn, and their philosophies (where here there is a small but necessary overlap with the previous chapter). I have made the decision to put these two topics into one chapter partly for space reasons, but also because I think they show very interesting contrasts: Popper's and Kuhn's philosophies are, in most popular philosophical discourse (for example Fuller 2003) represented as opposed in several fundamental aspects. They are, as any cursory glance at the science wars literature will show (see chapter 2), also supposed to be held in varying esteem by scientists themselves: While Popper is widely seen as the scientist's preferred philosopher, Kuhn is usually held to be very unpopular at least among the "hard" sciences (physics, chemistry, biology) on which I concentrate in the interviews. This juxtaposition of the two philosophers and their philosophies hides an enormous variation of actual philosophical opinion, as demonstrated also in chapter 4. Here I try to analyse the popularity (or lack thereof) of these philosophers together with the actual philosophical opinions as part of the social identity of scientific thought, which will be a theme that I revisit in the remaining chapters, especially chapter 7. Another developing theme is that of the *authority of philosophers* to talk about science, which may sound surprising given that scientists are supposed not to care about philosophers (according to the received opinion in the science wars, again), but makes sense as the appropriation of philosophical authority to draw boundaries around science and define scientific identities.

Chapter 6 shows the scientists' views on the value of simplicity in science, and the philosophical principle usually called "Occam's razor". This topic is rather curious because despite its general recognition as an important principle, it has received only marginal interest as a point of discussion within the philosophy of

science. Because I found that the way scientists write and talk about Occam's razor needs a slightly different classification than I found in most philosophical discussion, in this chapter I have added a classification to the justification of Occam's razor which I hope makes sense of the discussion more than merely a review of the philosophical literature which often focuses on the interpretations of simplicity rather than its justifications. Here again I try to categorise the different opinions I have heard and read regarding this topic and contrast it both to the philosophical discussions surrounding the razor, but also to the use it is being put primarily in popular science. I argue that, even more so than is the case for Popper, in popular science Occam's razor can be used as a philosophical authority with which to define boundaries and identities of science, and that curiously the discussion of *simplicity* itself is conducted differently to that surrounding *Occam's razor*. This is contrasted to the opinions I got in the interviews in our discussions on simplicity.

Chapter 7 deals with reductionism, which has been found an important topic within a set of popular science books that deals with a specific scientific controversy, but only rarely outside of that dispute and in the interviews. Within the life sciences, reductionism has been mentioned almost only by authors who took part in the Nature/Nurture or evolutionary psychology debate. While reductionism also featured in a couple of the physics books, I found the correlation between the Nature/Nurture debates and reductionism fascinating and therefore widened my popular science sample slightly to investigate this angle a little bit more. Through the comparison of the philosophical opinions that the scientists have regarding reductionism, and the way some of them identify themselves as either reductionist or non-reductionist, I argue that philosophical identifications notwithstanding, philosophical opinions can overlap substantially. The conclusion that reductionism is often about identity and the drawing of boundaries within particular debates is supported by the results I got from the interviews where reductionism was generally regarded as much less of an important issue than the protagonists of the Nature/Nurture dispute would suggest, and where again there was a wide range of different opinions on the philosophical side of what reductionism means for science in practice.

Chapter 8 then pulls together all the different strands and make the comparisons across the different philosophical topics and the two studies, to draw out my theoretical conclusions about scientific identities and boundaries and how philosophical topics can influence scientific thought which in turn influences how the philosophical topics are discussed. I also present my conclusion from three different angles, most importantly for the disciplinary setting to which my thesis is oriented there are the lessons of my study for social studies of science and in particular the public understanding of science. Finally, I offer some ideas that are of a philosophical nature, because I believe that my study will also have interesting lessons for philosophy.



## Chapter 2: Literature Review

|                                                                                           |    |
|-------------------------------------------------------------------------------------------|----|
| <b>1. Introduction</b> .....                                                              | 28 |
| <b>2. Why teach philosophy of science? Why philosophy of science at all?</b> .....        | 29 |
| 2.1. <i>PUS and the teaching of scientific method</i> .....                               | 30 |
| 2.2. <i>The science education community and scientific method</i> .....                   | 33 |
| 2.3. <i>Philosophy and science education, and the aims of philosophy of science</i> ..... | 34 |
| <b>3. Scientists' opinions on scientific method</b> .....                                 | 38 |
| 3.1. <i>Empirical studies by the science education community</i> .....                    | 38 |
| 3.2. <i>Philosophers and historians on scientists' opinions on methodology</i> .....      | 40 |
| 3.3. <i>Sociologists on scientists' opinions on methodology</i> .....                     | 43 |
| 3.4. <i>Scientists and literary cultures</i> .....                                        | 48 |
| <b>4. Conclusion and summary</b> .....                                                    | 52 |

### 1. Introduction

Studies on the philosophical opinions of scientists have come from several different disciplines, using different methodologies and have been performed for different purposes, which makes comparing them and pooling their conclusions very difficult. In this chapter I review this rather dispersed literature on the topics of what philosophy of science has to add to science, and what scientists' philosophical opinions are. I also give a very brief historical background on the recent scientist/philosophy relationship, the two cultures debates and the science wars, as it is with this background in mind that many more informal remarks on the scientists' opinions on philosophy are made. The science wars in particular is a theme that comes up at several places throughout this thesis.

The second section of this chapter looks at the arguments put forward for and against the use of philosophy of science in science communication, grouped by several disciplines. Research into science communication itself can have different targets, such as the public, school/undergraduate students and future scientists. There is some overlap between research in these areas, however the first is usually covered by the academic discipline of public understanding of science (PUS), the second is mainly the province of science specialists in education departments while the third is often left to the scientists themselves. There has been considerable research into the benefits of

philosophy of science to science communication primarily by the science educationists, however, the PUS literature has rather less tradition in bringing philosophy in. Another group with a possible interest in including philosophy of science in science communication is the philosophers themselves. Curiously, though, the current naturalistic tradition in philosophy of science seems to discourage advertising philosophy to scientists. It is naturalistic philosophy, however, that has sought to poll scientists' own lay philosophies.

The third section is a short review of empirical studies on scientists' philosophical work, and the justifications their authors brought forward for such work. A lot of historical work has been performed with the aim of finding out, for example, theory choice by scientists, though not many consider a scientist's own writing on why that theory was chosen. I have therefore included some references to studies from the history and philosophy of science that explicitly try to test philosophical ideas, even though it is not always clear how much of the scientist's own opinion has influenced the philosopher's conclusion. In the last part of the third section I examine what scientists publicly have had to say about philosophy in their own publications over the past 50 years through the "two-cultures" and "science wars" debates. This section falls slightly outside the main literature review, but is essential for providing the cultural background of the scientists/philosophy relationship. References to the science wars in particular will turn up in several places throughout the thesis and a quick introduction is therefore necessary. However, despite what some commentators from the science wars wrote, a lot of the comments made by scientists, even during the science wars were contributions against philosophy. There are many positive evaluations of and even contributions to philosophy of science, particularly among scientists of older generations, such as Medawar (1979, 1996) or Monod (1974) in France.

## **2. Why teach philosophy of science? Why philosophy of science at all?**

The question of whether there are any benefits to scientists being aware of philosophy of science is tied up with the question of what actually the purpose of philosophy of science itself is. In this section I review the arguments brought forward by the science

communication communities (which I will split into PUS and “science education”) on why philosophical considerations should be taught. I then describe some of what the philosophy of science community has to say on why their subject should matter to scientists.

### *2.1. PUS and the teaching of scientific method*

Among other things, the public understanding of science (PUS) literature has focused largely on what precisely the words *public*, *understanding* and *science* actually mean (Gregory and Miller 1998). The meaning of “science” is of particular interest when PUS scholars want to define the “scientific literacy” of the public in such studies as that of Jon D. Miller (1987) and the NSF (National Science Foundation) science and engineering indicators (NSF 2008, chapter 7, p.23). The kind of question such studies have used for assessing a respondent’s scientific literacy is usually something like: “In your own words, what does it mean to study something scientifically?” The assessment of this question presupposes that the coder has an idea of the answer him/herself. Miller, for example, attributes an understanding of the scientific process to respondents if their answer showed scientific study to involve

(1) the advancement and potential falsification of generalizations and hypothesis, leading to the creation of theory, (2) the investigation of a subject with an open mind and a willingness to consider all evidence in determining results, or (3) the use of experimental or similar methods of controlled comparison or systematic observation. (Miller 1987 p. 24 footnote 2)

The business of trying to assess the public’s scientific literacy has become unfashionable in PUS circles and is now seen as part of Brian Wynne termed the “deficit model” of PUS (Wynne 1992, 1993 for his original criticism of the deficit model, see also Gregory and Miller 1998, Miller 2001, Michael 2002), which sees the public as deficient in scientific knowledge, with the science communicator’s job being

to assess where the gaps are, and fill them. In his very influential study (1992), Wynne found that Cumbrian sheep farmers had more relevant local knowledge than the science experts who advised them about farming practises after the Chernobyl disaster. A more contextual approach challenged this deficit model in PUS by being critical of its assumption that increased scientific literacy will lead to an increased enthusiasm about science, and that all desirable flow of information between science and public should be from science to public. It was also found that increased knowledge about science does not translate into more enthusiasm about science (Turney 1996).

The caricature is often made that the deficit model applies to quantitative social science and the more contextualized model is associated with qualitative research (a characterization which Sturgis and Allum 2004 argue against). A more refined version of the deficit model seems to be making something of a comeback, but in any case, there are still plenty of situations where a deficit model is appropriate. When the question becomes “how do we best *teach* science”, contextual PUS does not provide (and does not necessarily want or need to provide) any answers (see also Bauer et al. 2007 for a history and reappraisal of the deficit model ideas).

Thus when J. D. Miller’s conception of scientific literacy about the nature of science is criticised no real alternatives are given for the purpose of teaching science. For example, Martin Bauer and Ingrid Schoon question Miller’s specific ideas on science as “basically Popperian” and remark that Popperianism has “no obvious claim to be a universal criterion by which to judge public understanding of science” (Bauer and Schoon 1993 p.143). Miller answers this charge by stating that this is precisely how scientists think about science. Since his PUS surveys are designed “to measure the level of public understanding of the scientific approach as understood and used by scientists”, Bauer and Schoon are missing the point: “The question was not created to gauge the public’s views on the philosophy of science” (Miller 1993 p.237). Miller’s response I would of course argue is inadequate because it leaves out exactly how the scientific approach is understood and used by scientists, though the point itself is reasonable that science should be taught in a way that scientists have themselves learned it, and a similar reasoning is prevalent in the science education literature

(section 2.2 below), which has tried to find out more about how scientists understand science.

Moving away from just conveying the epistemological workings of science, John Durant (1993) thinks that people have to understand not only how science works, but also how science *really* works. By this distinction he means that it is important to understand that scientists are not (only) acting according to rationalized scientific method, but also as members of a social community. Hence science education should make more effort to convey the social aspects of science, as well as the epistemological. However, just by calling the sociology of science “knowing how science *really* works”, Durant belittles the epistemological aspects of science (“knowing how science works”) as almost irrelevant.

More positively opposed to teaching scientific method is Henry Bauer (1994), who argues that even the idea of a clearly defined scientific method is misleading: Scientists in different disciplines and sub-disciplines employ different methods, and they are, after all, only human (see Knorr-Cetina 1995, 1999 for an empirical study of the use of different methods in different disciplines). The importance of this is not only confined to teaching the public: Through the “myth of a scientific method” they are encouraged to have an unrealistic view and expectation of science and scientists – but so are scientists themselves (p.40). However, the philosophies of science that Bauer criticizes, though they are intended to stand for all possible philosophies, are no more sophisticated than the rather simplistic Popperianism of J. D. Miller. Neither does he want to replace it with more sophisticated philosophy, but rather wants to retain teaching scientific method as an *ideal* (H. Bauer 1994 pp.19-41; 147).

The importance of understanding scientists’ view of philosophy for the purposes of PUS is underlined by the tendency of some PUS researchers and scientists to make sweeping generalizations about what scientists think. Henry Bauer makes the argument that philosophy can lead the public to have unrealistic expectations can be extended to scientists, but that needs to be examined. Similarly, J. D. Miller’s comment that “this is what scientists think” has not much empirical evidence to support it.

## 2.2. *The science education community and scientific method*

A related community which has drifted apart from PUS somewhat is that of science education, which in recent years has also developed an appreciation for researching how scientific method (in this field usually included in the umbrella term of NOS – Nature Of Science) should be taught. One of the main advocates of teaching history and philosophy of science is Michael Matthews, who edits a whole journal (*Science & Education*) devoted mainly to issues of history and philosophy of science in science education. In Matthews (1994), he argues that history and philosophy of science should play an important part in the science curriculum and goes on to explain its virtues with the example of teaching physics with the history and the philosophy of the pendulum. Nonetheless he also sounds a note of caution (Matthews 1998) that we cannot just tell teachers what scientific method is. Science teachers should recognize that there are “two, if not more, sides to most serious intellectual questions” (p.170). This sentiment reflects his judgment that science educators should themselves be well versed in philosophical debates, an attitude that they should then be able to instil in the training of science teachers (also Mathews 2003) In this he criticizes a similar position to the one Bauer and Schoon criticize in Miller and the NSF (see sect. 2.1 above). Matthews however takes the lesson that the people who communicate science should become adept at philosophical debate, rather than just becoming a “fence-sitter” because there is no clear resolution yet of the philosophical debates. Similarly, Koponen (2007) wants to enrich science teacher education (and therefore science education by extension) by adding more considered philosophical opinions, and possibly enlisting the help of philosophers themselves. In a similar vein Koponen and Mäntylä (2006) argue for the importance of epistemological issues of experiments in the student’s “own construction of knowledge”, especially by putting experiments back into their historical context.

A different tradition in science education takes a more “deficit model” approach to teaching NOS. Abd-El-Khalik and Lederman (2000a) argue that we cannot expect students to pick up sophisticated views on NOS without prior teaching in the issues of philosophy and history of science. In a study of science students and science teacher trainees focusing mainly on history of science, however, they find that that their study

“does not lend empirical support to the intuitively appealing assumption [...] that coursework in [history of science] will necessarily enhance student’s [...] NOS views.” (Abd-El-Khalik and Lederman 2000b p.1057). But when History of Science teaching was preceded by more general NOS instruction (on their sample of the teacher trainees only), they find that they showed some signs of displaying more adequate views, though more studies are needed. Again, a problem with these studies, as they are for their PUS equivalent, is that the coder judges on which NOS views are adequate.

In a similar study, Ryder et al (1999) look at the perceptions on NOS of undergraduate science students. As they are aware of the difficulties in coding the students for “scientific literacy”, the study aimed rather at providing an overall picture of students’ perception on science, rather than developing concrete opinions on where students’ ideas on NOS are deficient. Nevertheless, they identify some aspects on where science teaching could be improved, keeping in mind that these students will be the science communicators of their generation.

In an admitted advocacy of the devil, Davson-Galle (2006) explores some of the arguments against introducing the philosophy of science in science education, although he concludes against compulsory education in philosophy of science, which may unnecessarily restrict the student’s choice, and that students who are interested in becoming scientists themselves, should still learn about the nature of science.

### *2.3. Philosophy and science education, and the aims of philosophy of science*

From the Philosopher’s side there have also been some calls to become more active in science education and communication. In France the philosopher Dominique Lecourt has written a report for the Ministry of Education arguing for compulsory philosophy education in university science courses (Lecourt 1999). As an example for the desirability of better understanding of philosophy for scientists, he cites the American biologists’ experience with creationism. Through an inadequate definition of what a scientific theory is the scientists have been cleverly outmanoeuvred by the creationists in public debates. Creationists, for example, often claim that evolution is, like creationism, just a theory and therefore needs to be considered on equal terms,

invoking “tolerance” as a Mertonian-style norm of organised scepticism (see chapter 4 on scientists’ views on Mertonian norms and open-mindedness in particular). Usually (Lecourt cites S. J. Gould as falling into this trap), the biologists’ answer involves references to the truthfulness of evolution. A good example is:

Scientists most often use the word "fact" to describe an observation. But scientists can also use fact to mean something that has been tested or observed so many times that there is no longer a compelling reason to keep testing or looking for examples. The occurrence of evolution in this sense is a fact. Scientists no longer question whether descent with modification occurred because the evidence supporting the idea is so strong. (National Academy of Sciences 1998 p.56)

Here the creationists cry foul since that truthfulness was what they were contending in the first place. It descended into an “irritating and pointless debate in which the biologists let themselves be driven into a true philosophical trap” (Lecourt 1999 p.5, my translation)<sup>6</sup>. Although philosophy has no ready answer to such challenges either, it should at least give scientists an idea of the scale of their task, and an understanding of how simply dismissing creationists’ challenges makes them appear to disregard one of their own most cherished principles of open-mindedness.

Regarding the creationist situation, philosophers have been fiercely debating the benefits of involving philosophy in such disputes. Michael Ruse acted as an expert witness in the 1982 Arkansas case on the constitutionality of an act requiring balanced treatment between evolution and creationism in science teaching (Ruse 1998). This type of action (also Kitcher’s 1983 book on creationism) which makes a similar case to Lecourt has been heavily criticized by Laudan (1998), who argues that it will ultimately do more harm than good. The exchange between Ruse and Laudan has even been reprinted in an elementary philosophy of science teaching anthology (Curd and Cover

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<sup>6</sup> Débat irritant et sans issue dans lequel les biologistes se laissent prendre à un véritable piège philosophique.



1998), demonstrating how central the dispute over the role of philosophy of science is. Another discussion of the controversy, including interviews with both Kitcher and Laudan can be found in Callebaut 1993. Similar issues have surfaced in the recent (2005) Dover case featuring “intelligent design”, where philosophers of science have been expert witnesses again, with the addition this time of a philosopher and sociologist of science (Steve Fuller), appearing as an expert witness for the opposing (intelligent design) side (see Lynch 2006; an editorial for a special issue of *Social Studies of Science* concentrating on the case).

Chang (1999) and (2004) argues for the case that HPS scholars can make valuable contributions towards the practice of science itself and, through the recounting of how science got to where it is today and through providing an appreciation of epistemological problems faced by science, facilitate in the understanding of science of both scientists and the public. That, more specifically, philosophical literature on scientific method (as opposed to other issues in HPS) can make scientists better at their job is one of the central arguments of Lecourt.

Otherwise it is surprisingly difficult to find philosophers writing on the benefits of philosophy for scientists: There has, in recent years, been a perceptible shift and even a split in the aims in philosophy of science. Maybe the development occurred because philosophers saw that the deep epistemological problems of science were not as easily solved as the positivists or Popper imagined. Philosophers have instead turned to solving conceptual issues within particular sciences. Accompanied with this so-called “naturalistic turn” in the philosophy of science (Callebaut 1993, Fuller 1993, 2002), has been a marked decline of interest in finding a scientific method or definite demarcation criteria between science and non-science. The term naturalistic epistemology has been made famous by Quine (1994 [1969], in Kornblith 1994). The central idea is that “questions about how we actually arrive at our beliefs are [...] relevant to questions about how we ought to arrive at our beliefs” (Kornblith 1994 p.3). Next to naturalistic philosophy of science, the strong programme in the sociology of science (Bloor 1976, Barnes et al. 1996) can in a way be seen as continuing some of the same preoccupations and epistemological conclusions of a naturalised philosophy of

science (Yearley 2005 p.24 for example compares Bloor's 1976 argument directly with the naturalism of Quine).

The precise nature of naturalism and its place within epistemology and the philosophy of science is itself a matter of contention. Fuller (1993) argues that philosophy of science is, with a few exceptions, a normative enterprise and that in practice, naturalised philosophy of science is not "as radical as it seems" (p.4). By contrast, Callebaut (1993), who interviewed leading philosophers of science about the state of the subject, even subtitled his book "the naturalistic turn". What seems uncontroversial, though, is that with the demise of Popper and the positivists (and the rise of Kuhn), philosophy of science took a new direction into empirical evaluations of its claims, normative or not.

Accordingly, much recent philosophical work makes reference to how science was actually practised exemplified by historical case studies. The seminal work of Lakatos (1970, 1978), Laudan (1977) or Feyerabend (1993 [1975]) started the trend. Specifically, people have started to test philosophy by comparing it to what actually happens in science: Donovan et al. (1992) set up a program to test philosophical assertions on case studies in the history of science. Similarly, Hull et al. (1978) tested "Planck's principle" (that new scientific ideas only get taken up when the old generation dies) by looking at the writings of prominent biologists in the 50 years after Darwin's theory was published and checking whether people who were against the idea started changing their minds, or whether they simply died and left a new evolutionist generation to lead the field.

As philosophy is now more often perceived to give a description on how and why science is so successful, rather than telling it how it should be done, there is no concerted drive from the philosophy community to educate scientists on scientific method, (apart from the occasional philosopher who writes for *Science & Education*). Also, for issues like the creationism debate it can be argued that the lack of clear answers will only confuse rather than help (as Laudan 1998 did), so that there is no co-ordinated drive to educate the public, either (with exceptions like Kitcher 1984).

### **3. Scientists' opinions on scientific method**

Empirical studies of scientists' philosophies have been performed before, by scholars of different disciplines and for different reasons. For my purposes, the logical way would be to start with the PUS literature, which however has the least tradition in sampling scientists' views, which may be because of the rejection of the deficit-style models that inform much of science education. Philosophers themselves have studied scientists' opinions occasionally, this time not because of some deficit idea about scientists' lack of knowledge of philosophy, but conversely to inform philosophy itself better with actual scientific thought. Also there have been some studies trying to find out scientists' views on philosophy and scientific method through their own publications, in particular popular science books and scientific journal articles. Also, because they are aimed at the general public rather than other scientists, they also betray how scientists themselves think the nature of science or scientific method should be taught and publicly represented. The sociological studies on popular science books will get their own subsection due to their relevance to my own research.

Scientists also sometimes themselves try to portray how scientists generally view science, and I will also include a brief section on how scientists have generally interacted with philosophy of science in recent years, and I will include subsections on the "two-cultures" divide and the "science wars" as showing how philosophy has been viewed through some of the more vocal members of the scientific community.

#### *3.1. Empirical studies by the science education community*

Studies that focus on the philosophical opinions of science teachers and/or science students or pupils abound (for example Koulaidis and Ogborn 1989, Aguirre et al. 1990, Gallagher 1991, Lederman 1991, Driver et al. 1996). Practising scientists have not quite received the same level of attention, however, there are some studies that try to identify the philosophical opinions of the wider expert community, including, but not limited to, scientists themselves. The justification for such studies is one of practical concern: Science educators cannot educate science teachers to hold views of the NOS that differ greatly from what experts think about science, and for that it is necessary to

find out what their views are. Osborne et al (2003) take up that argument to conduct an empirical study of what the expert community think should be taught about the NOS. The experts for their study comprise 5 panels, each consisting of 4 to 5 distinguished scientists, philosophers and sociologists of science, science educators, science teachers, and PUS scholars. They conclude that their data underlines the case for more teaching of NOS and epistemology in school science.

A study that focuses explicitly on whether there is a consensus between philosophers of science on the fundamentals of NOS has been conducted by Alters (1997). Unsurprisingly, he finds that there is little agreement among philosophers, identifying 'a minimum of 11 fundamental philosophy of science positions are held by philosophers of science today.' (Alters also asserts that a philosopher's view on science correlates with his/her views on NOS.) This he sees as a reason for the failure of his discipline's longstanding goal for the teaching of NOS. However Alters' intention to develop a more appropriate measure of students' and teachers' views on NOS seems to have been largely abandoned.

Aware of the fact that there is no homogenous opinion coming from the Philosophers of Science, Matthews (2003) notes that the science education community itself must take care to be more aware of the issues in contemporary philosophy of science, even though they will find it difficult to engage with philosophers in serious scholarship in philosophy itself. To show that a critical attitude towards philosophy must be adapted he considers the enthusiastic uptake of positivism not only among philosophers but also the education community in the 1960s, or their subsequent misunderstanding of Kuhnian themes when they became popular. The fate of positivism suggests a more critical attitude towards popular philosophies of science on behalf of the educators was called for: The science educator must become a philosopher him/herself; it is not just enough to teach NOS without too much awareness of debates raging in philosophy. His argument echoes a view that is becoming dominant in the field, also evidenced by Ryder et al.'s arguments (1999), that advocates sensitivity in philosophical issues in teaching, as opposed to a simple deficit-

style model that aims to identify specific philosophical points students must be aware of.

### *3.2. Philosophers and historians on scientists' opinions on methodology*

#### *a) Historical*

There have been only a few studies that aim to clarify scientists' thinking on philosophical topics by looking at the historical records they left behind, or by interpreting their behaviour. In keeping with the descriptive nature of most recent philosophy of science, many philosophical ideas have been justified by looking at historical case studies. However, explicit references to what philosophical topics scientists consciously considered when doing their science are relatively (and surprisingly) rare. Thus, Donovan et al. (1988) edited a whole book that had the testing of philosophical ideas on historical case studies as its theme, yet none of the contributing papers seems to offer extended analysis of how a scientist consciously arrived at a particular decision, or how they rationalised them.

By comparing the reception of Einstein's famous predictions of light bending observed by Eddington, with the reception of his prediction of Mercury's perihelion advance, Brush (1989) tries to answer the question of whether a theory that makes successful predictions is better than one that does not, which is a claim favoured by many philosophers, including Popper. By looking at the technical writings of scientists at the time, Brush detects no clear evidence for the claim.

Whether philosophical ideas play any role in research is a question posed by Bowler (2000), which he tries to answer by looking mostly at examples of 19<sup>th</sup> century. Bowler starts with his suspicion that scientists rarely take philosophy seriously, and if they do, it usually does more harm than good. His evidence is that sudden changes in a scientist's worldview (such as Darwin's conversion to evolution) happen far too quickly to be the result of philosophical deliberation.

### *b) Contemporary*

Turning to the examination of contemporary scientists, an informal survey of the readers of the Institute of Physics magazine *Physics World* by the philosopher Robert Crease (2001, 2002) has been conducted, asking the respondents which of the entities in a list they considered “real”. The options ranged from stones to the Ptolemaic solar system and the direction of time. Crease intended to reply to an assertion made by the physicist John Polkinghorne (2000) that virtually all scientists adhered to a “critical realist” position. Although it is not easy to infer a scientist’s precise position towards realism from the answers to Crease’s questionnaire, it is at least clear that there “are sizeable fractions of the sample who disagree with [Polkinghorne’s] view”: 9% have even replied the Ptolemaic solar system to be real. Crease sees the necessity of an increased dialogue between scientists and philosophers partly for PUS:

By articulating the relationship between scientific practice to other kinds of human activities, a fully articulated philosophical position would make scientific judgments and decisions appear less abstract, strange and arbitrary to outsiders. This would help to re-establish a dialogue between the scientific community and its clients, supporters, academic interpretators and the public at large. There is a danger – not only to science but also to the public – if this dialogue breaks down. The so-called science wars are only the most recent manifestation of the breakdown of this dialogue. (Crease 2002)

Crease’s survey, being part of his regular column for *Physics World* is evidently very informal, there having been no effort to select a representative sample of physicists.

More formally, Bailer-Jones (2003) conducted interviews with scientists from the Open University, UK to find out their views on what a scientific model is. As there is an “increasing diversity of opinions” by philosophers on the topic of scientific models, Bailer-Jones finds it important to understand how scientists themselves think about models. This exercise is for her not about constructing a philosophy out of what

scientists think, but as a way of “gaining orientation”. For this reason she chose the interview method to gain an understanding of the individual’s personal point of view on the topic, “based on their experiences.”

In a more extensive, quantitative survey, (Stotz, Griffiths and Knight 2004 and Stotz and Griffiths 2004), a research group from the University of Pittsburgh was trying to find out how biologists are conceptualizing the concept of “gene”. The findings were compared with expected hypothesis such as the claim that evolutionists favour a different interpretation of genes than do molecular biologists. As opposed to Bailer-Jones’ “orientation gaining” study, the motivation for the Pittsburgh group was that of a case study of “conceptual change and its role in science”, as well as being interesting in its own right (Stotz and Griffiths 2004).

Unlike most of the historical studies that explicitly aim at identifying whether a philosophical hypothesis is tenable in actual science or not, these contemporary accounts analyze the scientist’s opinion, not their actions. A reasonable but unspoken assumption is that these opinions matter in the way they perform their science. There is, of course, a difference between using scientists’ philosophies to inform philosophy and reviewing opinions for orientation. Nevertheless, the fact that Bailer-Jones feels that there exists a “methodological requirement that philosophical stances towards models match the use of the term ‘model’ as used by scientists” (Bailer-Jones 2003 p.276), requires an examination of what philosophy of science is essentially supposed to be doing – this methodological requirement is not immediately obvious outside a naturalistic conception of philosophy of science. As for the Pittsburgh group, their motivation for clarifying the philosophical position of scientists is that, as scientists talk about genes in fundamentally different ways, the philosopher “has no alternative to examine what different biologists say and do” when trying to analyze them. Again, this style of philosophy is thoroughly descriptive, the philosopher has become a “conceptual analyst” (Stotz, Griffiths and Knight 2004, p.648).

### *3.3. Sociologists on scientists' opinions on methodology*

Among sociologists who examined scientist's opinions of scientific method, Mulkay and Gilbert (1981) interviewed a group of biochemists about how Popper's philosophy informed their science and their decision making on other people's science. They found that among the people who subscribe to Popper's philosophy there was a difference of opinion on which work is valuable: a scientist would say, for example, that his science is Popperian, and he would drop his views if they were falsified, while another scientist would criticize the first on exactly that point, that he did not drop his view, even though it had been plainly falsified. Their conclusion is that a distinction must be drawn between how scientists think they apply scientific method and how they actually do it, and that Popper's methodological rules are being interpreted very widely so as to comply with the scientists' ideas of science. Because Mulkay and Gilbert's study is directly relevant to my own findings, especially on scientists' views on Popper (chapter 5), I will provide a longer exposition of it and set it into the context of Mulkay and Gilbert's theoretical frame.

The wider study of which the Popper paper was a part of (Gilbert and Mulkay 1982, 1984) has become one of the more famous and influential works on (and using) discourse analysis within STS, and has even had a considerable influence on discourse analysis for social psychology (see Potter and Wetherell 1987). This work is based on a series of interviews with scientists who were involved in a particular field of biochemistry at a time when eventually Nobel-prize winning work was being made and discussed.

One of the main conceptual tools developed by Gilbert and Mulkay is that of the interpretative repertoire. Interpretative repertoires are "recurrently used systems for characterizing and evaluating actions, events and other phenomena" (as described by Potter and Wetherell 1987 p.149). Gilbert and Mulkay identify two major repertoires from their analysis of the scientists' discourse, an "empiricist" repertoire and a "contingent" repertoire. The empiricist repertoire is used mainly within the scientist's language in research papers and more formal talk, when they talk about established facts, observations, giving a logical and objective structure to the process of science.



By contrast the contingent repertoire is used when the scientists talk informally about the actual messy nature of scientific research, or when making jokes. Mulkay and Gilbert find, amongst other things, that scientists talk differently about correct beliefs than when they talk about incorrect beliefs.

Whereas correct belief is almost without exception portrayed as exclusively a cognitive phenomenon, as arising unproblematically out of rational assessments of experimental evidence, incorrect belief is systematically presented as due to the intrusion of social and psychological factors in the cognitive domain. (Gilbert and Mulkay 1982 p.384)

Due to the fact that many formerly accepted beliefs have turned out to be wrong, Gilbert and Mulkay argue that this kind of scientific rhetoric faces a problem, which people solve by asserting that, whatever we think we know now, the truth will eventually be found. This rhetorical manoeuvre, which Gilbert and Mulkay call the “Truth Will Out Device” or TWOD, is used very often by scientists to switch between the empiricist and the contingent repertoires.

In their study of the scientists’ views on Popper however, Mulkay and Gilbert do not demonstrate the two repertoires explicitly, although here as well they show how the scientists talk about Popper in different ways, depending on the circumstances:

Except in situations where Popper’s position is expressly repudiated, scientific conclusions where with which the speaker agrees are often portrayed as arising from actions which exemplify the Popperian rules and never as arising from actions which contravene those rules; while conclusions with which the speaker disagrees are often seen as arising out of contraventions of the Popperian rules but never as arising out of the conformity to the rules. (Mulkay and Gilbert 1981 p.403)

This is similar to my own results regarding scientists' opinions on Popper (chapter 5), where I will argue that for many scientists Popper has become a way of identifying with "proper" science: Good scientific practice is often seen as conforming to Popper's philosophy, whereas bad scientific practice is not (and people will of course usually hold that their own science is good, while that of their rivals possibly is not, as Mulkay and Gilbert show). Therefore, scientists' ideas of what Popper's philosophy is all about are in turn shaped by their own experience of scientific practice. My own results on scientists' views on philosophy will be interpreted in terms of boundaries and social identities which I will describe in the rest of this chapter.

A similar study has been made by Potter (1984) in analyzing the transcripts from a psychology conference and the participant's usage of testability and values from Kuhnian philosophy, and how that usage compares to Kuhn's predictions of what happens in science. He concludes that his study has illustrated, among other things, "how the same value may be used by the same scientist to perform quite different interpretative tasks" (p. 329), and how his results are comparable to Mulkay and Gilbert's study. Both Mulkay and Gilbert and Potter have used their sociological investigations of scientists' discourse to make philosophical conclusions: About the usefulness of Popper's normative claims (Mulkay and Gilbert), and the applicability of Kuhn's model of theory choice (Potter), and I will emulate this myself by offering some philosophical conclusions to this study in chapter 8.

The scientist's "lay philosophy" also has an impact in the practical issues of procuring funding. Here, the scientist has to persuade other scientists, or people with usually a scientific education, that their proposal applies scientific method. Balmer (1994) analyzed the usage of Popperian values in the decision to fund the Human Genome project in Australia. Since such a mapping job does not involve the falsification of hypothesis, as it does not even involve hypotheses as such, applications were rejected.

An analysis of the use of Popperian philosophy in astronomers' published technical works has been performed by Sovacool (2005). Sovacool finds that a number

of scientists directly made use of Popper for such purposes as the critiquing the testability of cosmology. Furthermore, he concludes that, even when there is only indirect evidence of Popper's ideas, the publications he reviewed 'indicate the widespread acceptance of Popperian ideas within the field' (p.60). A potential problem with Sovacool's study, however, is that quite often evidence for what he calls "indirect" references to Popper includes, for example, references to "testability": "McAlister repeatedly used language such as *prediction of stellar masses, theory selection, corroboration, testing theories, and validation by observation and experiment* throughout his article." (p.59, original emphasis). Though Popper used these terms and concepts extensively, he was not the first to use them and several other philosophies are consistent with the use of these terms.

#### *Studies of popular science books*

One way to analyze scientists' lay philosophy is through their own writings. Popular science books, serving primarily the interests of PUS in that they seek to explain science, in many cases need to explain what makes science special as an activity. It is therefore reasonable to assume that the scientist's philosophy will be revealed better in popular books than in the same scientist's technical writing.

Furthermore, popular science books also can have an enormous influence on other scientists (Paul 2004) by either being a comprehensive introduction to a new subject (such as Gleick 1988, the subject of Paul's article), or by having been written by influential scientists that command the respect of their peers (such as Weinberg and Feynman). As popular science books are often read by scientists and students about to become scientists, they should fairly well represent the most important opinions on scientific method as seen by scientists. (See also Broks 2006 for an introduction to popular science and an analysis of its role in PUS)

Of the surprisingly sparse research that has been done in the field of analyzing popular science as a literary genre, Fahnestock (1986) stands out with her classic study of comparing technical and popular writings on the same topic. In a later article (1998) she compares how the popular science styles (such as portraying the other side as a

scientific minority) are being used to argue academic disputes in a popular forum. Fahnestock singles out the effect that popular science writing has as an important factor in communicating science, but also warns that a popular style can distort the meaning of the science from texts that are technical by nature to texts that are used to persuade. Charney (2003), worrying that popular science has an enormous influence on the way the public perceive science, makes a similar point: Popular science often portrays science as the work of “lone geniuses”, thus misrepresenting the way science actually works.

Mellor (2003) has analyzed the boundary between science and non-science staked out by several popular science books. The books she analyzed had a particular interest in science fiction (i.e. *The Physics of Star Trek* – Krauss 1995 – and similar books); hence most of her work has gone into the boundary between science and fiction, rather than concentrating on scientific method or philosophy. Mellor’s argument focuses on the rhetorical aspects of the author’s boundary setting. Thus, through Hawking’s (1988) claim to “know the mind of God” through physics, “he appropriates God for physics and claims a universal and future role for physics” (p.531). The work that popular science books have achieved in defining science is, for Mellor, one of setting themselves apart by pre-empting other cultural activities like science fiction or religion. Little attention, however, is placed on any positive contributions these books make to a definition of what science is.

The study most interesting for my work, on how popular science books talk about scientific method has been proposed by Turney (2001). As “every narrative which relates something of what science has found, and how it was found, takes a view on the status of scientific knowledge, sometimes implicit, often explicit” (p.48), these books make a natural contribution to the NOS component that some PUS researchers want to see for their definition of “scientific literacy”. Turney intends to classify the philosophical position of the author according to a scheme developed by the science educators Driver et al. (1996) to interpret the NOS views of young people. They identify five main philosophical viewpoints: inductive, Popperian, Kuhnian, social constructivist and instrumentalist views. Turney proceeds to analyse some examples of

how popular science authors talk about the nature of science. Turney's work is meant to be an example of how popular science books could effectively be analyzed for how and how well it explains science, given that there is almost no working definition of what books should count as belonging to the genre. This proposed survey of how popular science books portray the nature of science has however not yet been done.

In his PhD thesis Adam Nieman (Nieman 2000) has examined the philosophical comments in popular science, coupled in his thesis with their pictorial representations. Nieman has focused his attention on a slightly different sort of popular science book than I have, taking into account also scientific fiction writing as well as taking a lot of his material from a particular collection of popular essays. In general I would concur with his conclusion about the "pithy" character of many philosophical comments in popular science acting as boundary work for the scientists (see also my own results in chapter 4). Nieman has not gone into an examination of particular philosophical themes, which will occupy my chapters 5 to 7, nor did he compare his results with what scientists think, apart from that I believe that my own study complements his conclusions, and therefore a longer discussion of his work and how it relates to mine will follow in chapter 4 below.

### *3.4. Scientists and literary cultures*

As a way of finding out not only the scientists' lay philosophy, but also their attitude to philosophy of science in general, a good way to start is to consider the literature scientists wrote as a direct response, or even contribution, to philosophy. However, it needs to be noted that this literature is hardly going to represent scientists in general, as only those who feel strongly enough about those issues are motivated to make their views public.

A distinction needs to be pointed out first: There should be little doubt that most scientists think that there is something that sets science apart from other activities, and that usually means a belief in some sort of scientific method (see chapter 4). Whether they believe that philosophy, or at least academic philosophy, has any important say in the matter is not so obvious. Regarding the implications of any argument on whether

philosophy of science should figure in PUS the question on how philosophy in general is regarded by scientists is crucial. Overemphasising philosophical issues could possibly alienate scientists should philosophy be regarded as unimportant, as could an emphasis on the wrong philosophies. The “science wars” (see below) attest to the sensitive nature of the topic among scientists.

### *“Two cultures” and philosophy*

There are possibly two stories to be told, scientists’ attitude towards philosophy and their attitude towards “literary culture” in general. At the time C. P. Snow wrote his influential essay *The Two Cultures* (Snow 1993 [1959]), philosophy of science was closely followed, and even shaped, by the most influential scientists themselves. Holton (1984) has remarked that scientists at that time grew up on philosophical work by earlier famous scientist-philosophers such as Mach. Heisenberg himself wrote a monograph on the philosophy of physics, (1989 [1958]) and in this he followed a tradition of intense philosophical interest among influential scientists of the preceding generations like Mach, Poincaré and Einstein (see also Heisenberg 1969). In the biological sciences, similarly influential scientists like J. B. S. Haldane (1931) have also extensively written about philosophy<sup>7</sup>. Among the philosophers of science at the time, such as the Vienna circle or Russell, there was an affiliation more towards science than to traditional philosophy, which resulted in the Vienna circle’s and Wittgenstein’s famous rejection of metaphysics.

This is to be contrasted with the effect C. P. Snow saw regarding “literary culture”. Whether Snow was correct in his analysis or not, it seems certainly true that by the time Holton (1984) wrote, philosophy of science had lost much of its appeal among many scientists, with eminent scientists like Feynman or Weinberg drawing their inspiration from other sources, or even belittling philosophy of science as irrelevant (Weinberg 1993). Why that shift occurred is a fascinating question to which I have not found an answer. It is, however, interesting to note that the shift seems to have

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<sup>7</sup> It is also worth noting that one of the few popular science books of my sample to focus extensively on the philosophy of biology was written by another heavyweight of mid 20<sup>th</sup> century biology, Ernst Mayr (1997).

occurred roughly at the same time as philosophy of science turned naturalistic (ca.1960 – 1970), though that could well be a coincidence.

The two stories that I mentioned at the beginning of the section are only confusing if we identify philosophy of science as part of the literary culture; C. P. Snow could have been right (and I think he was) in his analysis if philosophy of science was regarded as part of science, rather than of literature, and if the writings of Heisenberg (and many others, such as) are anything to judge by, it seems to have been. It is not true, though, that younger scientists only have bad things to say about philosophy. A two-culture style divide between science and philosophy of science seems not to have been present very much, at least not when C. P. Snow was writing. The earliest attacks at the presumptions of philosophy of science were fired by quotes like Feynman's famous quip that "philosophy of science is as much use to a scientist as ornithology is to birds" (for which I however have not been able to find a reference, suggesting that this may be one of the many apocryphal stories that surround Feynman), and even that is mild compared to the abuse the "two cultures" threw at each other after Snow's speech.

#### *The science wars*

Who really started the science wars, and when, is contentious, Theocharis (2001) for example fiercely claiming priority over the more famous publication by Gross and Levitt (1994) which usually starts reviews of the science wars (see also Jardine and Frasca-Spada 1997, Hacking 1999, Brown 2001, Segerstråle 2000 for histories and analyses of the science wars, Koertge 1998 for the reaction of some philosophers in support of the science side). Gross and Levitt organized a conference about what they thought of as the "Flight from Science and Reason" in 1995 (Gross, Levitt and Lewis 1996). Some of the contributors to that conference and the resulting book were philosophers themselves (e.g. Susan Haack 1996, Bunge 1996), suggesting that the Gross and Levitt's irritation was not directed at philosophy itself, but rather specific issues in science and technology studies, including constructivism and postmodernism, which means they are not hostile to philosophy but only to philosophies they disagree

with. This they share with most philosophers themselves, of course - what was unusual however was the violence and personal nature of the attack.

Philosophy as a general discipline, however, was not spared by two other publications, Weinberg 1993 and Wolpert 1992. While Weinberg, who makes the effort to keep abreast of developments in philosophy of science and even endorses some philosophy (on the sleeve cover of Hacking 1999 for example), claims that philosophy only benefits science when it rescues it from even more dangerous philosophies (1993, chapter 7), Wolpert goes even further by claiming that philosophy is completely irrelevant to science: “Not only are scientists ignorant of philosophical issues, but science has been totally immune to philosophical doubts” (Wolpert 1992 p.108).

The incident that has attracted most attention, however, was Alan Sokal’s hoax article in a special science wars edition of the cultural studies journal *Social Text* (Sokal 1996a) and his subsequent admission of the hoax (Sokal 1996b), which brought the debate into the national newspapers. As Sokal’s attack was mainly focused on French philosophers like Bergson, Lacan, Kristeva and Latour, he teamed up with the physicist Jean Bricmont to publish a book that tries to explain why they think these philosophers are scientifically illiterate (Bricmont and Sokal 1997, *Impostures intellectuelles* – the chapter on Bergson has been removed in the American edition, Bricmont and Sokal 1998). The reaction in France was equally venomous and spilled into the public media as well. One reaction to the debate was a book titled *Scientific Impostors* (Jurdant 1998), featuring papers by philosophers as well as some scientists, mostly arguing for reconciliation and in defence of philosophy. While the people Sokal and Bricmont attack are mostly French Sociologists of Science (e.g. Latour) and postmodernists (e.g. Kristeva, Irigaray etc.), they also devote a chapter to “regular” philosophy of science, including critiques of Popper, Kuhn and Feyerabend.

It is worth keeping in mind however that postmodernism’s fashionable phase was relatively short and there has since been a trend away from it that makes Sokal and Bricmont’s picture of French philosophy seem very outdated (Matthewman and Hoey 2006). On the other hand, as discussed above, in France sociologists of science such as



Latour are much more perceived to be part of the mainstream of philosophy of science, the hostility that divides the disciplines in Anglophone philosophy appears to be far less pronounced. Bouilloud 2003 charts the reception of the Sokal affair in France, see also Jeanneret 1998. A detailed list of most publications relating to the affair can be found on Alan Sokal's webpage (Sokal 2007).

At least, keeping the two cultures discussion in mind, it seems clear that even before there has long been a simmering discontent among scientists for the literary disciplines like philosophy, which is greatly enhanced when these disciplines start talking about something that scientists feel *they* should be the experts in, i.e. science. As soon as it is perceived that philosophy and sociology of science are no longer staffed by (or even for) scientists, like the positivists and Robert Merton were clearly perceived to be, scientists feel they are being lectured about something they ought to know much better about. Where the acrimony for the science wars comes from is understandable. Gieryn (1999) analysed this conflict in more detail using his concept of boundary work which will be described in the next chapter.

#### **4. Conclusion and summary**

In this chapter I have reviewed the various literatures on why philosophy of science is seen as important for science education or the public understanding of science, and how researchers in this tradition have occasionally tried to underscore that point by looking at scientists' own views of matters in the philosophy of science. This has also included some studies that look at representations of the nature of science in scientists' own writings, both in the technical literature and in popular science. Finally, I have tried to chart some of the historical developments in the public relationship between scientists and philosophy, because I believe it gives a flavour of people's expectations of what scientists think of philosophy, and in that sense provides a background for this thesis as well as provide some initial hypotheses that this thesis can look into.

Comparing all these studies into scientists' philosophical opinions with each other and with the public image of the scientist/philosophy relation is very difficult because the studies were performed with many different methodologies and with different aims

in mind. Added to that, several studies singled out particular philosophical topics, such as realism (Crease 2001, 2002), models (Bailer-Jones 2003) or Popper (Mulkay and Gilbert 1981), and/or concentrated only on a particular scientific discipline, for example Nieman 2000 on physics, or Potter 1984 on psychology.<sup>8</sup>

Nevertheless, there are some themes that emerge. Most prominent is the idea that scientists admire or are at least well aware of the philosophy of Karl Popper. This is one of the interesting results from Gilbert and Mulkay's study which has shown that Popper was mentioned often enough spontaneously in their interviews. Similarly Sovacool's research shows that Popper's philosophy is frequently referenced by astronomers (even though his methodology cannot establish the proportion of astronomers who cite him, as it is based on a keyword search for "Popper"). This accords well with the public image science warriors have given of science during the science wars, and this is one of the reasons why I will be looking at the reception of Popper in this thesis (chapter 5). Another recurring theme is that of scientists using philosophy in their boundary work (see chapter 3) which is a theoretical viewpoint that I will be partly adopting myself. Nieman claims to have identified boundary work in the philosophical remarks in popular science, as does Gieryn himself when he analyses the science wars as a boundary dispute (Gieryn 1999, epilogue).

Mostly though the conclusions that I derive from this literature review are negative rather than heuristic: It seems that there has been some research into scientists' philosophy which however has uncovered little empirical support or refutations of all those frequently uttered opinions on what scientists think about philosophical topics. Although my thesis will add another study which necessarily can only look at a limited sample of philosophical topics, scientific disciplines with similar limitations in research methodologies and theoretical grounding, I still have sought to be able to address more points more systematically than the previous studies I have

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<sup>8</sup> Here of course my own study will be similarly limited, because even though I have tried to include several disciplines as well as several philosophical topics, there still are plenty of topics and disciplines I had to leave out.

pointed to, to add some new interpretations to how scientists talk about philosophical topics, and to ground some of the assumptions that are usually made about scientists and their attitudes towards philosophy.

# Chapter 3: Boundaries and Identities

- 1. Introduction ..... 55
- 2. Summary of the boundary approaches ..... 56
  - 2.1. Thomas Gieryn: Boundary work and the demarcation problem..... 57
  - 2.2. Boundary objects ..... 59
  - 2.3. The influence of the boundary approaches on science and technology studies..... 60
  - 2.4. Criticism of the boundary approach ..... 62
- 3. Summary of the social psychology approaches ..... 64
  - 3.1. Social representation theory ..... 65
  - 3.2. Social identity theory..... 67
  - 3.3. Criticisms of the social psychology approaches ..... 70
- 4. Social identity frameworks applied in STS..... 72
  - 4.1. Contrasts and similarities between boundary work and social identity ..... 72
  - 4.2. What a social identity perspective can add to the boundary work theme (and the possible limitations) ..... 75

## 1. Introduction

This chapter will put together two strands of research that both come out of a tradition of analysing people’s talk and have therefore both been described as theoretical underpinnings of “discourse analysis”, the concepts of boundaries from science and technology studies, and social identity and social representation frameworks from social psychology. The chapter will give a general introduction to two approaches to boundaries familiar to science and technology studies; Gieryn’s (1983, 1995, 1999) and to a lesser extent Star and Griesemer’s (1989) concepts of boundaries. The next section will introduce two theoretical approaches from social psychology, social representations and social identity theory. I will then compare and contrast the two traditions, and identify where the two approaches complement each other.

In the later chapters of the thesis I will be making use of both approaches, and, while I will not necessarily use them interchangeably, I will nonetheless show where the connections exist, and where both sets of terminology can be used for the same phenomenon. For example, chapter 7 will interpret the usage of the philosophical concept of reductionism in a particular set of popular science books both in terms of social identity theory and in terms of boundary work, and I will

follow Nieman (2000) in interpreting general philosophical remarks (chapter 4) as boundary work. In the concluding chapter 8, I will revisit the comparisons and the ways they apply to the examples considered in this thesis.

Discourse analysis is, like many theoretical terms in the social sciences, a very disparate concept which has received many different treatments by many different authors (Potter and Wetherell 1987). It concerns itself with the discourse of people under study, as it is that which is the only thing that is available to the researcher directly, rather than the person's real thoughts about a subject (see also Yearley 1988 on the difference between peoples' accounts of their actions and their actual actions, and how that informs sociological approaches such as discourse analysis). His/her actual activities are of course directly observable, though, even if they are faithfully recorded, they still need to be interpreted by what we think the person intended to do. Moreover, it is often the language itself that allows people to clarify their thinking for themselves and for others (see Billig 1987 for an influential defence of the study of rhetoric for social psychology). This thesis is inspired by the discourse analysis approach, which as surveyed by Potter and Wetherell's book can also make use of the theoretical frameworks from social psychology that I will describe in this chapter.

## **2. Summary of the boundary approaches**

The metaphor of symbolic boundaries between social groups has been used extensively in the social sciences, from Durkheim to Weber and Simmel (see Lamont and Molnar 2002 for a review of the concept). Within Science and Technology Studies two particular approaches have achieved prominence, Gieryn's (1983) concept of "boundary work", and the concept of "boundary objects" elaborated by Star and Griesemer (1989). In this section I will first provide a brief description of Gieryn's approach to boundaries, followed by an even briefer one of Star and Griesemer's concept, as their work will be slightly less relevant to my thesis. I will then discuss the impact they had on STS, on sociology generally, and indeed, on each other. Finally I will end the section by providing some thoughts of where I think the boundary work approach is yet still deficient and in need of further elaboration.

### *2.1. Thomas Gieryn: Boundary work and the demarcation problem.*

In his seminal paper (Gieryn 1983) on boundary work, Thomas Gieryn shows how attempts by scientists to demarcate science from non-science or pseudoscience can be interpreted as a rhetorical device to apportion authority to the scientists concerned and to protect the autonomy of science from interference by politics or ideology, as well as rival accounts from other scientists. These demarcation attempts are found “in scientists’ attempts to create a public image for science by contrasting it favourably to non-scientific intellectual or technical activities” (p. 781). Through this demarcation rhetoric, scientists construct “boundaries” between science and whatever activity they contrast it with. Gieryn bases his analysis on earlier sociological studies on ideology, drawing in particular from Geertz’s 1973 work. The boundaries that scientists draw in their discourse can vary according to what science is being contrasted with. Gieryn considers three cases that exemplify three different facets of boundary-work. In all these cases boundary-work puts a favouring contrast between science and non-science.

The first example concerns the popularisation work of John Tyndall. For Tyndall, both religion and technology presented serious threats to the autonomy and proper functioning of science, though these threats were of entirely different nature. Tyndall advanced definitions of science and of what is not science that are not always completely complementary to each other, depending on whether he was arguing against technology or religion. But in both cases his definitions threw a favourable light on science. His second example was the gradual exclusion of phrenology as a proper science in the early 1800s. Here, a group of prominent scientists used their authority and resources to construct a boundary that excludes from science a rival group that also considers itself as scientific. These boundaries were drawn by trying to show phrenology’s political and religious (and therefore unscientific) motivations, by questioning phrenology’s theoretical vagueness that seemed to remove it from proper empirical scrutiny, and by accusing phrenology on relying on popular rather than the scientific community’s support for its popularity. Thirdly, Gieryn considers boundary work between science and its popular consumption and political interference, as worked out in a 1982 report by the Committee on Science, Engineering and Public Policy of the (US) National

Academy of Sciences. The report included boundary work by “demarcating the university-based production of ‘basic’ knowledge from its technological consumption and application”, to insure that the legislators “may accept its conclusions and follow its recommendations” (p.791).

His examples, Gieryn concludes, “have a common rhetorical style: attributions of selected characteristics to the institution of science for purposes of constructing a social boundary that distinguishes ‘non-scientific’ intellectual or professional activities” (p.791). Boundary work as a stylistic and ideological device is used by scientists to expand the authority of science into other professions (as in the case of Tyndall), to monopolise the professional authority and resources through excluding “rivals from within as outsiders with labels such as ‘pseudo,’ ‘deviant,’ or ‘amateur’” (p.792), shown by the case of phrenology. Finally, he also considers a third example of boundary work which protects scientists from the undesirable consequences of their science through the demarcation of politics and science (or “finding scapegoats”).

Gieryn collected his ideas and some of the case studies in more detail in a later book (Gieryn 1999), and in his entry to the Science and Technology Studies Handbook (Gieryn 1995). In the book (1999, introduction), he also goes further with his explanation of the cartographic metaphor: A real map is drawn up with the particular needs of its readers and authors (a map for motorists is unlikely to feature hills, which however may be of interests for pedestrians). Likewise the metaphorical map on which the boundaries of science are drawn is also continuously negotiated, redrawn and differently relevant according to who is using and who is drawing the map. This adds an interesting dynamic to the metaphor which failed to be adequately clarified in the original paper. It is worth keeping in mind, as with any metaphor, how far it can be made to stretch, and whether if any particular extension of a metaphor has been successful, this reflects a fundamental discovery about the subject at hand, or if we are just deluding ourselves by putting too much stock into the metaphor. In this particular case I believe Gieryn’s metaphor is safe, as I indeed think that the nature of boundaries between social groups are continuously changing, both in their physical extent and in relevance towards who is looking and defining, and when. The drawback is that the metaphor

emphasises, for the more casual observer of Gieryn's work, the static and objective nature of boundaries. In this I think Gieryn's choice of metaphor leaves him at a slight danger of being easily misunderstood. In fact, as Gieryn himself comments, exactly the same social processes can be (and have been) analysed using a number of other metaphors: it can be seen as a game, a network or even as a "category in a classification" (Gieryn 1999 p.6).

## *2.2. Boundary objects*

Within science studies, the metaphor of boundaries has also been used by Star and Griesemer (1989), who introduce the notion of "boundary objects", which enable crucial intercommunication between different groups. A boundary object enables different groups to communicate by "providing a common coin" (p.413) among the groups. The groups that Star and Griesemer are concerned with here are not rivals that, as in Gieryn's schema need to protect their interest against outsiders, but rather different groups that may have different values, norms and aims, but nevertheless need to work together. Through the analysis of boundary objects, Star and Griesemer want to examine how different viewpoints can collaborate on scientific enterprises, without there necessarily being any "consensus imposed by nature" (p.388). The central tension they identify is that despite all the differences between the groups involved in it, science must find a way of creating consensus.

Star and Griesemer illustrate their concept of boundary objects and the type of situation where they are used through the case study of Berkeley's Museum of Vertebrate Zoology in the early 20th century. The smooth functioning of the museum depended on a variety of different groups with distinct values, aims, beliefs and abilities. Among them they examine the directors, the benefactor of the museum, the scientists, the amateur collectors, the university administrators, and so on. Boundary objects are defined as

those scientific objects which inhabit several intersecting social worlds [...] and satisfy the informational requirements of each of them. Boundary objects are objects which are both plastic enough to adapt to local needs and the constraints of the several parties employing them, yet robust enough to maintain a common identity across sites. (Star and Griesemer 1989 p.393)



As an example of a boundary object that allows for the intercommunication of amateur collectors and the museum scientists they point to the standardised protocols for the collection and preparation of specimens. They stress that these objects are not merely an imposition of one social group's vision and values on another as these rules may at first suggest, but are negotiated bridges to enable the scientific work of the museum to proceed.

### *2.3. The influence of the boundary approaches on science and technology studies*

Gieryn's metaphor of boundary work, and his analysis of the rhetorical strategies employed for such boundary work also lend themselves easily to other analyses where professional or social groups draw boundaries to differentiate themselves from others. Staying within the general context of science studies, this could for example include demarcation attempts between scientific disciplines, or domains. Also, the rhetorical character of boundary work is further emphasized by work within the discipline of rhetoric. Holmquest, for example, has argued "that rhetorical scholars can and must play part in the resolution of boundary disputes and that concrete case-studies of boundary-work may deepen criticism of argument" (Holmquest 1990).

That Gieryn's concept of boundary work can easily be adapted to fit into a more general sociological framework, rather than just one from the domain of science studies, is reflected in a literature review by Lamont and Molnar (2002), which looks at several other similar approaches from the domains of different social sciences, such as anthropology, sociology and social psychology. Gieryn was not the first to speak of boundaries in social science, and there are therefore several different approaches that use similar language. Added to the general proliferation of the metaphor in the social science is the fact that Gieryn's work itself occasionally finds usage in sociological investigations outside science and technology studies, two examples here are Bishop (1999), and Lamont (1992). Aware of Lamont's work, Gieryn himself writes that his work could be useful for a broader analysis in other social domains, though he has not himself developed that thought any further (1999 p.34).

Gieryn's concept of boundary work has become a fairly standard conceptual tool in the analysis of scientific groups and their rivalries. Especially for the study of people who for some reason or other fall outside of the mainstream of science, an analysis of the boundaries of science and how those are drawn up is useful. The concept has proven helpful especially in the sub-discipline of PUS, where boundary work is shown to be performed when scientists themselves try to communicate their subject, as they first have to establish their own credibility to talk authoritatively about their subject. Gieryn's study of Tyndall's boundary work through his popularisations may serve here as an example, or Cassidy's (2003, 2005, 2006) examination of boundary work in popular evolutionary psychology.

Gregory's (2003) work on the "excommunication" of Fred Hoyle and his "life from space" idea is a good example of the usefulness of the boundary approach to the analysis of scientific controversy, and on the usefulness of boundary work analyses in PUS. Although boundary work is not the only conceptual tool used by Gregory to make sense of the episode, it does feature prominently. Gregory describes how the astronomical community marginalized Fred Hoyle and his collaborator Chandra Wickramasinghe while they were promoting Hoyle's "life from space" theory. Gregory argues that the boundaries that were erected to separate them from the scientific orthodoxy included intellectual as well as physical and social separation, i.e. that they were excluded from scientific literature as well as from scientific friendship networks. As the communities separated, communication between them took on more and more of the characteristics of popularisation, as the conceptual gap to be bridged grew so that even more fundamental things had to be explained. This, Gregory argues, is typical for marginalized scientists:

An excluded scientist's only means of reaching other scientists is through popularization. Thus popularization a key tool in boundary work – it acknowledges and defines its audience as some separate "other" – and communication that functions across such a separation serves, irrespective of genre, as popularization (Gregory 2003 p.26).

Boundary work therefore can be argued to happen on both sides, the people who do the demarcating as well as by those who have been demarcated out from the orthodoxy. Both sides can engage in rhetorical (as well as social and physical) demarcating to draw boundaries, though of course for slightly different reasons. Nevertheless, in both cases people build up a professional identity that excludes the other party.

A complete theoretical and conceptual combination of the exclusive and demarcating aspects of boundaries as described by Gieryn and others with the collaborative aspects of boundary work as explored by Star and her colleagues, still seems to be some way away. Both traditions illustrate different aspects of boundaries found within the social analysis of science. They can be erected and used as an interpretative strategy by scientists to distance themselves from groups they perceive as conceptually threatening in some way. A group or a group member can draw a rhetorical boundary that excludes other groups' claims to competence in their area, thus exerting or trying to exert some sort of control over their epistemic authority. In the other tradition a boundary is seen as a given division between social groups that, while working together, view the world and the object of their collaboration in fundamentally different ways. In this view a boundary is not something created to establish "epistemic" authority, but rather something to be overcome to create scientific cooperation. In these two traditions boundaries are different concepts, with different types of functions and different types of groups that they delineate. That these two different uses of the boundary metaphor can co-exist so easily within science and technology studies is surprising, and shows either the need for some deeper thought into how they interconnect, or otherwise a reappraisal of the general usefulness of the boundary metaphor.

#### *2.4. Criticism of the boundary approach*

Surprisingly, given the considerable impact that Gieryn's work has made on science and technology studies, there has been very little criticism made of it. It is interesting though that most of the publications that I could find that use or reference to boundary work almost always supplement it with some other theory.

Thus for example Gregory's work as discussed above, mainly uses Lewenstein's (1995) "web" model of communication.

This leads me to at least one possible shortcoming of boundary work theory: Even though its concepts and metaphors do seem to apply to a lot of situations in science and even beyond, it never really seems as if people think it describes the whole picture. It is mainly for this reason that I hope to be able to supplement the boundary approach with those from social psychology. In doing so I hope to be able to find an explanation of why people behave in the way that Gieryn describes. Very often, sociological work that invokes boundaries stops at the identifying stage: the author identifies that boundary work has taken place, references to Gieryn, and then moves on to other observations.

Gieryn himself of course does go a little bit deeper, though still falls short of actually explaining individual behaviour other than marking it as an exercise of asserting power and authority.

Boundary-work is strategic practical action. As such, the borders and territories of science will be drawn to pursue immediate goals and interests of cultural cartographers, and to appeal to the goals and interests of audiences and stakeholders. Insider scientists use boundary-work to pursue or protect several different "professional" goals. (Gieryn 1999 p.23)

Gieryn reiterates that boundary work can take on several forms, depending on what exactly is achieved by it. The aim of boundary work can be that of expulsion, that of expansion or that of the protection of autonomy (pp.15-18). By making this explicit, Gieryn attempts to give an explanation of why people are doing boundary work, to establish their authority, say. Yet in a way it merely shifts the need for an explanation towards another level: It is still not quite clear why people would want to establish the authority of a science, or of their own particular branch of science. A typical explanation here, which is usually inferred rather than made explicit, is that this kind of behaviour somehow serves the actors' self-interest. But precisely how that self-interest is served, and why it is served best through exactly this kind of action, is in some cases not quite clear. A physicist conceivably benefits from excluding bodies of knowledge that directly threaten him/her, as is the case in

Gieryn's case study of the cold fusion debate (1999, ch.4). But exactly why would a natural scientist benefit from excluding the social sciences, as in his case-study about the debates on whether social science is a science at all during the foundation of the National Science Foundation (ch.2)? Moreover, do these cases have any deeper underlying motivations in common, or do they just fall under the same metaphor?

Generally, why do scientists feel the need to protect their professional goals at all? Why would they feel they are under threat? I believe that going a bit further into the social psychological level can offer some insights, and hopefully complete or at least add to Gieryn's work.

By having his framework limited to science and technology studies, it can easily appear that boundary work is peculiar to the behaviour of scientists. As Gieryn himself has argued, and as Lamont (1992) has shown, that is not the case. However, if the work is to be more relevant to a wider sociological audience, then it needs to be shown what the similarities are between the boundary work in completely different social spheres, and for that we need to go down an explanatory level again into motivations and identities.

### **3. Summary of the social psychology approaches**

As I have argued in the introduction to this chapter, I see my contribution rather as a continuation of science and technology studies discourse analysis in the tradition of Gilbert and Mulkay (1984), and my choice of theoretical framework will be one which I think neatly ties into the tradition of boundary work theory from science and technology studies. Interestingly, of the two approaches I will introduce, only one (social representation theory), has seen a developing presence within science and technology studies (and especially PUS), while the other closely related one based on Henri Tajfel's (1981, Tajfel and Turner 1985) work on social identity, is as far as I know relatively overlooked in the field<sup>9</sup>.

Those two approaches should not necessarily be seen as competing accounts and have even been combined (see Doise 1998), as I will outline below. I will refer to them separately as social representation theory (SRT) and social identity theory

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<sup>9</sup> Michael Hogg has recently written a chapter on using SIT to analyse trust in risk management (Hogg 2007), which is however in an area peripheral to science and technology studies.

(SIT), but will also refer to both as social psychology approaches, for brevity. In this section I will first provide a brief outline of the social representation theory, followed by a more detailed account of social identity theory. I will then present some of the criticism that has been levelled at these approaches.

### *3.1. Social representation theory*

Within science and technology studies, the concept of social representation is making a growing impact. For its main proponent, Serge Moscovici (1993, 2000), social representations are very much relevant to sociology of science and have been developed further, in the context of the public understanding of science by Farr (1993), Bauer and Gaskell (1999) as well as by Washer (2004, 2006). This particular use of social representation theory certainly seems very fruitful as applied to the public representations of scientific concepts. A further development, which I would like to outline in this chapter, would be to widen the social representations approach to generally the scientists' own representations of scientific concepts, and for the purposes of my thesis, of philosophical concepts.

A social representation is the shared conception a social group has about a concept. The social representation is not necessarily the particular opinion of a member of the community; however, his/her opinion is derived from it. Duveen (2000) gives the following example of a social representation: when asked to point to the location of Vienna and Prague on an empty map of Europe, most people who grew up during the cold war would place Vienna (wrongly) to the west of Prague – this being a mental image of Europe's geography which reflects what we know of its political division. This demonstrates the power that the social representations that people have of Europe in shaping how they think about it in different ways. Another example, given by Potter and Wetherell (1987 p.139), is that of the conservative party: Our opinion on it is usually based on what we hear about it from the press and discussion with friends, but rarely directly from the party itself.

Social representation theory attempts to understand how social representations get formed and develop with time, and with the interactions of the people who hold these representations. As the representations are meant to be shared within the group, they ease communication within it: "social representations are first and foremost intended to make communications in a group relatively non-problematic

and to reduce the 'vague' through a degree of consensus among its members" (Potter and Wetherell 1987 p.151, original emphasis).

When people are confronted with a concept they are unfamiliar with, they look to *anchor* it in familiar representations and it is through this process that social representations develop a particular dynamic: the original representation is modified by this as much as the new one acquires its meaning (Moscovici 2000 p.150). This also leads to a certain conservatism in that new representations always have some aspect of already known things. Moscovici sees the same processes going on within Kuhn's (1962) concept of paradigms (see chapter 5), which are also very conservative in this sense (p.151). Social representation theory is thus very much meant to be a social psychology of knowledge (Duveen 2000), and therefore has found a moderate amount of application in science and technology studies. Because of the influence that the popular media have on our perception of things, especially when we are not experts in them, much research on social representations for the purposes of PUS focuses on how science and scientific concepts are portrayed by the media (see for example Farr 1993 and Washer 2004, 2006). Moscovici himself set this trend at the beginning of his career with a discussion of the social representation of psychoanalysis in France (Moscovici 2007 [1961]).

Bauer and Gaskell (1999) elaborate this further with their own development of social representation theory particularly with PUS in mind. They lay a particular stress on the distinction and relation between the representation, the object and the subject ("the carrier of the representation"), and how this relation changes over time. What exactly the subject is (i.e. who holds the representation), is in many cases not very clear. In most PUS studies it is simply assumed to be "the public", but this itself is problematic enough and topic of some debate within PUS (Gregory and Miller 1998). When social representation theory tries to study a more narrowly defined group, the precise identity of the group is often a representation of the group itself. For example, the study of how a group represents something will need to make distinctions on who belongs to that group. A biker is who the community of bikers says is a biker – yet in order to study the social representation of bikers by bikers, we would have to know who to study in the first place. This circularity is one of the main objections that Potter and Wetherell have (1987 p.143) towards social representation theory, and it is very similar to a difficulty about my own

research that I have mentioned in chapter 1 (footnote 2): As this thesis is asking scientists to define science, I had problems deciding who to include in the study as “proper” scientists.

### *3.2. Social identity theory*

Henri Tajfel defined social identity as “the individual’s knowledge that he belongs to certain social groups together with some emotional and value significance to him of group membership.” (Tajfel 1972 p.31, quoted in Hogg and Abrams 1988 p.7). The groups that are the focus of Tajfel’s concept of self-categorisation are, rather than factual or demographic groups such as race or class, psychological entities, i.e. groups that people emotionally feel they belong to. This can include categories such as race and class, but not necessarily so.

Social identity theory is first of all about how these psychological divisions are created, and how they in turn create distinct social groups. Social identity theory uses the insight from several experimental studies that social groups with a strong identity can form even if there are no important differences between them (the “minimal group paradigm”, see below), and aims to explain this phenomenon by providing the mechanisms of group identity formation.

[The social identity approach] considers identity and self-definition to mediate between social categories [...]. [A]s a social psychological approach it explores the psychological processes involved in translating social categories into human groups (Hogg and Abrams 1988 p.17, italics in original)

A fundamental pillar of SIT is that group members enhance their own self esteem by conforming to the group identity by positively evaluating their own and their group’s characteristics as opposed to outsiders. A consequence is that SIT can explain how these groups and individuals interact and produce effects such as intergroup stereotyping, prejudice and discrimination. In fact the theory has been used to serve as a response to these practices, as an understanding of how prejudices and discrimination arise can serve in formulating a response to them.



### *Self-categorisation*

The formation of a person's social identity relies on the "accentuation effect". Experiments have shown that when people categorise things into distinct groups, they tend to overestimate the differing features, and underestimate features they have in common<sup>10</sup>. A particular example that Hogg and Abrams give is that of the rainbow, which is a continuous spectrum of colours going into each other, but it is very hard for us not to think of 4 to 5 distinct colours with distinct boundaries next to each other.

This effect is incorporated into social identity theory by Tajfel, as it shapes the conceptualisation of the social categories that people feel they belong to. The social categories into which people classify themselves (and others), are often more continuous in character than people perceive. This process, which Tajfel calls "self-categorisation", thus has the effect that people tend to stress the similarities they share with other members of the group, and downplay their differences. On the other hand, differences with other groups, or members of other groups, tend to be accentuated.

Self-categorisation at once accomplishes two things: it causes one to perceive oneself as 'identical', to have the same social identity as, other members of the category – it places oneself in the relevant social category, or places the group in one's head; and it generates category – congruent behaviour on dimensions which are stereotypic [...] of the category. (Hogg and Abrams p.21)

Self-categorisation also leads to a link with SRT, as group members' perceptions of their own group can be thought of as a social representation, and analysed as such (see Doise 1998, Augoustinos 2001). Linking social categorisation with social representations also shows how representations can be used for social positioning.

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<sup>10</sup> Hogg and Abrams quote studies such as Secord 1959 and Tajfel and Wilkes 1963.

### *Ingroup characteristics and stereotypes*

As each group has defining characteristics, i.e. values, norms, beliefs, even physical attributes in common, part of the process of self-categorisation is that one convinces oneself and others that one possesses these characteristics. The perception of one's own characteristics as conforming to the group's defining characteristics, and one's positive evaluation of those characteristics, enhances the group member's self esteem and own perceived social standing within the group. Similarly, because it is important for people to positively evaluate the group they are part of (the "ingroup"), and because through the accentuation effect the differences to other groups ("outgroup") are particularly emphasised, the perceived negative characteristics of the outgroup are seen as particularly pronounced. The ingroup has thus formed a stereotype of the outgroup. Stereotypes are usually exaggerations of perceived characteristics that a group is supposed to have. Social identity theory is intended to explain how stereotypes are formed and why they are indeed overwhelmingly concentrated on perceived negative characteristics of the outgroup. Tajfel (1981, preface) explains quite consciously that his thought has developed to explain discriminatory behaviour of the worst kind, himself having come to Britain after spending time in German prisoner of war camps and losing most of his family during the second world war (see EAESP 2006 for a short biography). Such identity markers, both positive evaluations of the ingroup and negative stereotypes, form another possible connection here with social representation theory as described above. Doise (1998) outlined that these characteristics can themselves be thought of (and analysed) as social representations held by the ingroup.

### *"Minimal group paradigm"*

One area of interest for a social identity framework would be to find out what would be the minimal amount of difference between groups so that the effects of social categorisation and stereotyping show themselves, an area of research now usually called the "minimal group paradigm". To that end social identity researchers have devised experiments to try and test the behaviour of groups of people who have no apparent differences with each other apart from being split into different groups by the experimenters (see Tajfel's 1982 review of intergroup relations

research for an overview of results and Hogg and Abrams pp.49-51). Participants were for example divided into groups by aesthetic preferences, coin tosses or by other means irrelevant to the tasks asked of them. The subjects had generally no interaction with each other, and remained anonymous. Tajfel (1982) describes experiments (for example Billig and Tajfel 1973 p.23), that show a consistent bias towards members of the ingroup. In general the results suggest that the formation of a group identity does not necessarily require that there are any real differences between the ingroup and the outgroup.

Hogg and Abrams explain the importance of the minimal group paradigm for social identity theory:

we would wish to argue that individuals in minimal group studies are categorising themselves in terms of the social category provided by the experimenter, and that such a process of categorisation (of self and others) accentuates intergroup differences on the only dimension available – the allocation of points. (Hogg and Abrams 1988 p.51)

This phenomenon, together with self- and social categorisation does seem to lead to the kind of intergroup competition that social identity theory wants to explain.

### *3.3. Criticisms of the social psychology approaches*

#### *Social representations*

As already mentioned above, the main criticism that Potter and Wetherell have for social representation theory is the exact boundaries of the groups, who belongs to them, and why, are not given, and crucially that they are themselves social representations of those groups. The need to identify those group boundaries is not only hampering the social researcher, but also brings circularity into the argument (1987 p.156).

Directly contrasted to social representations Potter and Wetherell's preferred approach is that of the interpretative repertoires (see chapter 2). Repertoires are not bound to particular groups, as the same individuals have recourse to the different repertoires in different situations. Their second point is related to that, because for them one of the major plus points to interpretative repertoires is that the same

people can draw on different repertoires in different situations, something which is not quite so easy with a more static view of social representations:

Because people go through life faced with an ever-changing kaleidoscope of situations, they will need to draw upon very different repertoires to suit the needs at hand. From this theoretical perspective what is predicted is exactly variability rather than consensus (Potter and Wetherell 1987 p.156)

Newer formulations of social representations theory, such as that of Bauer and Gaskell (1999), emphasise the development of the representations and the groups that hold them, which may help answer Potter and Wetherell's point. As I will show below in chapter 7 on reductionism, it can well be argued that people can see themselves as belonging to different groups at different times and thus adjust to their groups' respective social representation of things. That people can hold conflicting beliefs about something, and how they deal with it, is also explained by Moscovici's concept of cognitive polyphasia (Moscovici 2000 chapter 7). This is shown neatly by Gieryn's example of Tyndall's demarcation efforts.

### *Social identity theory*

In his critical review of social identity theory, Brown (2000) identifies several specific areas or corollaries which he thinks have been problematic, mostly because experimental evidence is inconclusive on these points. I will only describe one example, which is the relationship between group identification and ingroup bias, by which he means that it is reasonable to conclude from the body of the theory that "there should be a positive correlation between strength of group identification and the amount of positive intergroup differentiation (or ingroup bias)" (p.753). This however seems not to have been shown experimentally.

Brown of course also discusses attempts by adherents of SIT to address these problems, and in general concludes that while the discussion on some of them will doubtless continue, SIT clearly shows a lot of promise in areas where the groups are in conflict with each other, and that "its main contribution here is to complement those theoretical explanations that locate those disputes in objective clashes of interest" (p.768). But even after this cautious criticism, further research on SIT is

still encouraged to iron out these difficulties. In any case, Brown acknowledges that SIT has very rapidly been accepted into the mainstream of social psychology, a development he finds so marked that he even suggests it may be of interest to future historians of science.

A different type of weak point has been pointed out by Huddy (2001), who argues that, at least from a political psychology viewpoint, while traditional SIT has been developed from experiments in the laboratory, there exists no real inclination by social identity researchers to study real world situations “complicated by history and culture” (p.127), and that research in that area is insufficient. Again, this is a point that does not fundamentally attack SIT, but rather calls urgently for more research to be done to real life situations. This is a conclusion also drawn by Huddy, which I feel is just as pertinent for the case of political psychology as it is for science and technology studies, if SIT is ever going to become an important framework for either field.

#### **4. Social identity frameworks applied in STS**

In this section I will attempt to show where the similarities between the two concepts of boundary work and social identity lie, and how they will fit into my thesis. I argue that SIT is a possible tool for understanding (at least some of) the processes that underlie the boundary work being done by scientists as Gieryn has identified them. I will also try to understand Star and Griesemer’s boundary objects as social representations held by the different groups. I am certainly convinced, with Lamont and Molnar (2002), and more explicitly argued in Lamont 2001, that these four concepts interconnect.

##### *4.1. Contrasts and similarities between boundary work and social identity*

In their review of literature on boundaries in the different social sciences, Lamont and Molnar review the STS concepts of boundaries (both in Gieryn’s and Star and Griesemer’s senses) as well as the social identity approach. Though there are of course even more concepts in other social sciences that are similar, and talk about both boundaries and identities abound in most social sciences (with many slightly different meanings), the two traditions that I am concerned with here have to my knowledge rarely been compared directly.

At first glance it is quite striking that the social psychology approaches (both SRT and SIT) are in fact concerned with boundaries themselves, as the groups differentiate themselves (SIT) and hold their own representations of concepts (SRT). The process of self-categorisation is in many ways comparable to the drawing of a boundary around the group. Similarly, boundary work as explored by Gieryn can be seen to be about the formation of group identity by the conscious and unconscious differentiation with the outgroup. It is an almost curious omission that the boundary metaphor, which after all is quite prominent in the social sciences, has not appeared more often in discussions of SIT.

But there are some contrasts as well. Most prominent is the methodology used in studies of SIT and the boundary work studies. While boundary work is usually studied through discourse analysis, or case studies of real life situations, work in SIT has been criticised for remaining largely theoretical and experimental. SRT does not have this problem, as it has been widely used with all manner of different methodological approaches (Bauer and Gaskell 1999), and as such has also received a wider treatment as a framework for discourse analysis in Potter and Wetherell (1987), as shown above in section 2.

#### *Gieryn's work and social identity theory*

Through the discursive manoeuvring that Gieryn identifies in his analysis, scientists like Tyndall can also be seen as building up a group identity of scientists. This is achieved through the demarcation of what it means to do science, i.e. members of the group need to fulfil certain characteristics in order to belong to that group. Likewise, people who do not conform to these ideals are stereotyped.

The boundaries that scientists draw are varied and overlap, so for example a physicist can draw several boundaries around what he/she thinks is physics, science, worthwhile knowledge, in the sense that many physicists think social science is not proper science, but nevertheless some form of worthy knowledge production – some of the respondents in this study for example argued that philosophy, or even social science is not scientific, but still produces worthwhile knowledge. In terms of SIT, the profusion of boundaries that people set around all manner of their social activities is quite easily understood, as SIT maintains that people always have

multiple and continuously shifting social identities, reflecting the fact that we can think of ourselves as being members of different groups, simultaneously and at different times of our lives. In chapter 7 I will argue that the biologist E. O. Wilson's changing attitude to the philosophical concept of reductionism can be explained by the different scientific groups that he belonged to during the different stages of his career.

Interpreting Gregory's example of the marginalisation of Fred Hoyle and his work in terms of social identity theory may also make sense. After it became clear (for whatever reason) that the life from space theory was not going to be accommodated that easily with scientific orthodoxy, it also became socially disadvantageous to be associated with Hoyle and his collaborators. Hoyle has thus become a target of discriminatory practices and prejudices which expanded beyond the sphere of the original controversy over life-from-space (Gregory 2003 argues that it may have been the life from space issue that denied him a share of the Nobel Prize that maybe should have been awarded to him for his earlier non-controversial work). The scientific community effectively drew their boundaries around their own identity of what being a good scientist is like, and consequently Hoyle's perceived non-scientific attributes were accentuated as well as his redeeming features (like his earlier work) were played down. Likewise Hoyle's reaction was to build on his own identity as a proper scientist in which the rest of the scientific community was not conforming to good scientific practice.

This brief attempt at a reinterpretation of the boundary work idea from a social identity viewpoint does not add anything to the original analysis in this case and is only here intended to show that the two concepts of boundary work and social identity are talking about the same kind of phenomena. In the concluding part of this section I will explore what the benefits could be of such a reinterpretation.

### *Boundary objects as social representations*

Another possible parallel here is that it may be possible to understand Star and Griesemer's concept of boundary object as a social representation held by the different groups. As a group becomes aware of a new concept, it will try to make sense of it by anchoring it to concepts that are already understood within the group (Moscovici 2000, see above). The mechanisms by which this can be achieved, as

outlined by social representation theory, help in answering Star and Griesemer's question on how different groups with remarkably different aims, values and worldviews can nevertheless achieve effective collaboration. The boundary objects that they identify can be seen as social representations that the individual groups make relevant for themselves by anchoring them to concepts, norms and values that they are already familiar with, and that they can understand. In fact, the description of such intergroup collaboration and understanding is exactly one of the questions that Moscovici hoped to be able to explain through his theory.

#### *4.2. What a social identity perspective can add to the boundary work theme (and the possible limitations)*

I have argued that the social identity perspective and the boundary work idea are two different and complementary ways of interpreting the same kinds of phenomena. This may have several advantages. One is that there exists an enormous amount of work in the social identity approach, and it seems that developing similar explanatory devices and tests of the theory from a boundary work perspective is in a sense re-inventing the wheel unnecessarily, and possibly even limiting a very interesting and powerful sociological theory to the unfortunate marginality of science and technology studies. Similarly, some of the criticisms that SIT must face may also need to be addressed by the boundary work approach. I will on the other hand not speculate on what possible benefits the boundary work literature can bring to SIT, though identifying if there are any lessons to be learned may be an interesting project, I feel it lies outside my remit for this thesis. However, in light of the previously mentioned lack of real life situations that have been analysed in SIT terms, the relative profusion of boundary work studies could be interesting for SIT. Conversely, SIT's heritage of experimental evidence, with all the obvious strengths and shortcomings for experiments in STS and social science more generally, could possibly add some entirely new and exciting perspectives to boundary work studies.

Also some concepts developed within SIT, such as prototypicality (a set of group norms which are seen by its members as prototypical of the group, see Mummendey and Wenzel 1999, Waldzus et al. 2004), may be an example of a theoretical development of social identity theory that has no direct counterpart in the boundary approach, at least not in Gieryn's original paper. In his book (pp. 65-



114) Gieryn touches on a case study which could be made sense of as ingroup projection, the debates in the US surrounding whether social science should be seen as a proper science (and therefore qualify for funding by the National Science Foundation). In a way the natural scientific world strained to define itself as more prototypically scientific than the social sciences. The example may offer an insight into the development of the boundaries of different sub-groups, and possibly start explaining and even predicting the dynamics of such groupings and their boundary work.

One other possible advantage of bringing social identity theory into the study of boundaries in science is that the explanatory level of SIT lies in the analysis of individual and group aspirations. In a way, SIT explains what the boundary work perspective observes. This point however impinges on the wider debate of bringing (social) psychological explanations into sociology, which is related to the discussion of the value of reductionism in science, which will be further discussed in chapter 7 as one of the philosophical concepts looked at in further detail. Where the level of explanation should lie for the various disciplines is an important question, but one I will not discuss here.

In the social identity perspective the actual drawing of boundaries can be more easily seen to be two separate acts, that is the drawing of a positive self-concept around the ingroup and the stereotyping of the outgroup(s). Boundary work in Gieryn's sense can too often just be seen in a negative sense of excluding certain features from the socially desirable group; however just as important is the positive idea of what is required to be part of it. Taking for example the demarcation of science from non-science problem that concerns Gieryn in his original article (and me in this thesis), the question of demarcation can be put two distinct ways: "what is science" and "what isn't science". These two ways of formulating the demarcation problem are for instance visible when contrasting the short defining "pithy" remarks on what is science found in the books which I will review in chapter 4, with the otherwise quite similar remarks on Occam's razor which I will analyse in chapter 6, which are used to define things that are not science. The two kinds of boundary work, exclusive and inclusive, have of course also featured in general sociological work on symbolic boundaries. Lamont (1992 p.10) traces the

idea all the way back to conceptions of symbolic boundaries from Durkheim and Simmel. However, with Gieryn, the precise meaning of what a such a boundary is actually supposed to be gets lost somewhat in the intuitiveness of the metaphor; here again SIT provides a way forward by theorising on the precise nature of exactly what the boundaries consist of (positive group concepts, prejudices, etc. which ultimately can themselves be understood as social representations). SIT offers at least the beginning of an understanding of where the boundaries come from and how they develop. This can provide not only an understanding of how the boundaries between groups are erected and maintained, but also how nevertheless communication between groups can occur.

In general, once we identify where the boundary and the social psychology approaches intersect and complement each other, we can make use of the work that has already been done in either area to heuristically inform approaches to the other. This is particularly pertinent in case of social identity theory because it has a considerable amount of previous theoretical and experimental research to draw on, which can provide guidance on which direction the idea of boundary work and boundary objects in science and technology studies could fruitfully be developed. I have already sketched in this section some of my ideas of how this might be done.

In the following chapters, I will be making use of all four of the concepts covered in this chapter. Through this way, the thesis will also highlight some of the ways where the different approaches can complement each other. I will for example analyse the use of some philosophers and philosophical concepts – such as Popper (chapter 5) and Occam's razor (chapter 6) – as boundary objects and also as social representations, which will highlight some of the mechanisms and features behind the formation and use of boundary objects (chapter 8). Similarly, I will be interpreting the use of reductionism in popular science (chapter 7), as well as the differing receptions of Popper and Kuhn (chapter 5) by scientists as being about drawing boundaries around science and particular disciplines, but also about being icons around which scientists can collectively identify. I will show how identification can highlight and explain differences in the presentation and interpretation of these philosophies among scientists that go beyond simple disciplinary boundary drawing, and instead explores and explains some of the tensions apparent in scientists' discourse about philosophy.



## Chapter 4: What is and isn't science

|                                                                                       |     |
|---------------------------------------------------------------------------------------|-----|
| <b>1. Introduction</b>                                                                | 79  |
| <b>2. Philosophical introduction</b>                                                  | 81  |
| 2.1. <i>Induction</i>                                                                 | 81  |
| 2.2. <i>Hypothetico-deductivism: logical positivism and falsificationism</i>          | 83  |
| 2.3. <i>Social norms and the social side of science</i>                               | 85  |
| 2.4. <i>The development of philosophy of science in France</i>                        | 86  |
| <b>3. Science defined in the books</b>                                                | 87  |
| 3.1. <i>Philosophical asides</i>                                                      | 88  |
| 3.2. <i>The different implicit philosophies</i>                                       | 91  |
| <b>4. Comments on what is science in the interviews</b>                               | 101 |
| 4.1. <i>Initial reactions</i>                                                         | 102 |
| 4.2. <i>Attitudes towards philosophy, and education received on scientific method</i> | 103 |
| 4.3. <i>The philosophical opinions</i>                                                | 106 |
| <b>5. Conclusions and summary</b>                                                     | 120 |
| 5.1. <i>Boundaries and repertoires</i>                                                | 121 |
| 5.2. <i>Three types of opinions on science and scientific method</i>                  | 123 |
| 5.3. <i>French and English conceptions of science</i>                                 | 124 |
| 5.4. <i>Summary</i>                                                                   | 127 |

### 1. Introduction

This chapter will present some of the general categories that the scientists have used in discussing the nature of science. Chapters 5 to 7 will discuss reactions to specific philosophies, philosophers and philosophical concepts, and some of the themes discussed here will be examined in more detail there. In this chapter I am more interested in finding the scientists' ideas about what is and is not science. In a sense this chapter is intended to set the scene for the following ones: Here the scientists' ideas about how science works are presented, so that they can then be compared and contrasted to their ideas about specific philosophical points. This will be a survey of the different ideas people had about what science is, what interviewees have said about how they related these opinions to philosophy itself, and about how they came to these opinions.

I have not tried to find out any individual scientist's overall coherent position. In the popular science books, philosophical comments were mostly scattered throughout the length of the books; they often fulfilled specific rhetorical functions with respect to the science that was being presented rather than being intended to

definitively state the author's view on the nature of science. In the interviews, where I have gone more into detail of the scientists' positions through directly asking and discussing the question of "what is science", the answers were often quite short and qualified by statements to the effect that they did not really know the answer, that the question is difficult, or that they do not know much philosophy. Therefore it is of course excusable that the interviewee would think on the spot, and possibly revise his or her opinion on thinking about it more deeply. In some cases where there have been interesting contradictions between philosophical statements made by individuals in both the books and the interviews they have been flagged up in this chapter, however I do not intend to read too much into them.

This chapter will first give a quick introduction to the philosophy of science, focussing on the concepts with which I will interpret the scientists' own philosophical ideas, and also give a quick introduction to the different way the French- and English-speaking philosophical traditions have developed, to aid the comparison between the UK and the French scientists in the interviews. The following section will present the different ways in which the nature of science was defined or explained in the popular science books, starting with an observation on the nature of these comments, and then presenting the most common ones divided into topical categories, although there is some overlap between the categories. The fourth section presents the answers from the interviews regarding philosophy and the nature of science. I will start with describing the interviewees' initial reactions to the question "what is science?", then I will present a brief account on how they themselves learned scientific method about what science is themselves, and how they would like these issues to be taught to future generations of scientists and the public. I have mostly used the same interpretative categories in the two sections to make comparison easier. In any case, I have found that the comments in the books and in the interviews were fairly similar, both in their type and their relative distributions. Where I felt there has been a major difference between books and interviews I have flagged it up.

The type of work philosophical comments in the books performed will be analysed as boundary work, where brief and concise philosophical statements are very often made to label a particular part or episode of science as properly scientific.

Mostly these statements are needed when the science in question is perceived by the author as controversial and therefore in need of further justification, or conversely when the author needs to show something as non- or even pseudoscientific. Concerning the philosophical content of scientists' philosophical comments, there were many different ideas about science and what it is, some of which came up very often even if they have no particular prominence in the philosophical literature (such as science being replicable or "common sense"). I will class the comments into three general categories: induction, hypothetico-deductivism and the social side of science, which I will argue scientists do not necessarily see as competing accounts of science.

I will also contrast them with the distinction between the empiricist and contingent repertoire identified by Mulkay and Gilbert (see chapter 2). I will argue that the study shows especially in the interviews that the scientists held sophisticated philosophies which explicitly acknowledged that science had objective and empiricist elements as well as a human, social side, and that both sides are not necessarily seen as incompatible or competing. Regarding the comparison of philosophical opinions held by scientists in France and the UK, there was not much difference other than that there was a possible generational contrast not in philosophical opinion, but awareness of French philosophy: senior French scientists were more aware of the French philosophical tradition, though they did not necessarily have discernibly different philosophical ideas about science.

## **2. Philosophical introduction**

This introduction is not a complete survey of the history of philosophy of science. Instead it will concentrate on those philosophical ideas which I have seen represented in the books and heard in the interviews. It will also include a brief introduction to the development of philosophy of science over the last century in France, because its differing tradition in this area was one of the motivations for including a sample of French scientists in the interview stage.

### *2.1. Induction*

One of the earlier and enduring ideas about scientific method is that the scientist observes the world through experimentation and that from repeated observation and

experimentation he/she will be able to draw conclusions about generalised behaviour such as the laws of nature. The idea that we observe a lot of things with a particular property A that have another particular property B and from that we can infer that all things with property A have property B, is called inductive inference. The name most associated with induction is the philosopher Francis Bacon who is regarded as one of the philosophical founders of scientific method and has been enormously influential (Bacon 2000 [1620]).

In a broader sense, the idea that observing the facts precedes speculation about theory is a theme common to the British empiricism of Locke, Berkeley and Hume and the later very influential tradition of logical positivism (see below).

One of the most famous problems with induction is that there is absolutely no certainty about it. Observing that any number of things with property A also have property B is no guarantee that *all* A's have property B. The most influential example here is about the observation of white swans – no matter how many swans we observe to be white, the next swan could always turn out to be black (this example was very well recognised by the interviewees – see chapter 5 – and associated with the philosopher Karl Popper, described below and in chapter 5).

Bertrand Russell has memorably described the problem like this:

The man who has fed the chicken everyday throughout its life at last wrings its neck instead, showing that more refined views as to the uniformity of nature would have been useful for the chicken (Russell 2001 [1912], p.35)

This problem, often called *the* problem of induction, or *Hume's problem*, was famously formulated by David Hume (1999 [1748]), who took it not really to mean an end to inductive reasoning, but merely a limitation of it.

It is sometimes argued that we can reasonably expect a “uniformity of nature” (in Russell's terminology), not least from the fact that the method of induction has proven very successful so far. However, that would be an essentially *inductive* argument for induction and therefore unsatisfactory. Otherwise we could stipulate the uniformity of nature as a separate metaphysical statement which needs no

further justification. The main problem here is that even then nature is maybe usually, but not always uniform (as the poor chicken has found out).

Some influential attempts around the problem of induction that still retain the importance of inductive inference in science have often focussed on finding ways in which we can at least find out a measure of how certain we can be about confirmations. The view that successive confirmations of a hypothesis will increase the probability of that hypothesis was Russell's (and logical positivism's) preferred approach. A variant of that approach which tries to calculate these probabilities using Bayes' theorem is still very influential today (see Earman 1992; Howson and Urbach 1993).

## *2.2. Hypothetico-deductivism: logical positivism and falsificationism*

Hypothetico-deductive accounts of science stress that there is an interplay between theory or hypothesis and evidence. As the name suggests, we start with a hypothesis from which we deduce consequences which we then go and test or try to observe. On its own hypothetico-deductivism is not incompatible with inductivism (as the hypotheses must come from somewhere), and indeed it has become one of the cornerstones of the logical positivist method of science as well as that of Karl Popper's falsificationism (Popper 2002 [1934], 1992 [1963]), which however *is* incompatible with inductivism.

Here I depart from the usual introductions to the philosophy of science by giving hypothetico-deductivism its own section rather than splitting it up between logical positivism and falsificationism, and may therefore give the term more prominence than is usual in philosophy. This is because many of the scientists quoted below would themselves prefer to emphasise the hypothetico-deductive nature of scientific method much more than the issues of confirmation or falsification. This in fact reflects one of my conclusions for the whole thesis, which is that the actual scientists' categorisations of philosophical problems are in many ways very different to the philosophers' categorisations, even if the underlying philosophical problems are the same (see chapter 8).

Logical positivism, also often called logical empiricism, started through a group of philosophers based in Vienna in the 1920s (and who dispersed into the English speaking world during the 1930s) commonly called the *Vienna Circle* who



aimed to construct a general scientific methodology and even worldview which was derived from the earlier work of Russell and Wittgenstein. Logical positivism was popularised in England by Ayer's very influential 1936 book (Ayer 2001 [1936]), and to an extent Ayer's version of positivism became so influential that it is often overlooked that there was a big development over time and difference of opinion between the major positivist philosophers (see Reisch 2005). The general tradition of logical positivism (and approaches derived from it), though started in Austria and Germany, became very much the mainstream of English speaking philosophy of science through the dispersal of most of its members (as well as some of its critics such as Popper and Feyerabend (1993 [1975]) to England and America immediately before and during the war.

Regarding scientific method, logical positivism, adhering to an inductivist view in the manner of Russell, required that we seek to verify hypotheses through observation. Hypotheses or statements which are not (in principle) verifiable are held not only to be unscientific, but also literally meaningless. Because of the problem of induction, Ayer differentiated between "strong" verifiability and "weak" verifiability, where weak verification means that "it is possible for experience to render it probable" (Ayer 2001 [1936] p.18).

Karl Popper (whose philosophy I will explain in greater detail in chapter 5), proposed to solve the problem of induction by claiming that science should seek to find *disconfirming* instances, so that instead of finding hypotheses that we can (at least reasonably) be sure are true, we can at least find out definitively which hypotheses are false, and move on from there. Similarly, for Popper it was falsifiability rather than verifiability which defined science (though he did not go as far as calling non-scientific statements meaningless). Ultimately Popper's philosophy suffered from a remarkably similar deficiency, namely that it is in fact not quite the case that we ever can disprove a hypothesis once we take into account that all hypotheses as well as observation statements require background knowledge, and that it is never sure whether the hypothesis or the background assumptions (or even the assumptions behind the observation statements) are at fault for a falsification. The argument was famously made before Popper by Duhem (1991 [1904]) to argue against inductivism and Quine (1980 [1953]) to argue against both

positivism and falsificationism (see also Gillies 1993 and the philosophical introduction to chapter 5).

### *2.3. Social norms and the social side of science*

Next to philosophies of science that concentrate on the logical nature of the relationship between hypotheses and evidence, and evidence and the truth, some characterisations of science also highlight that science functions through scientists as social beings. The social organisation of science can have several effects on science, good as well as bad. In his famous characterisation of science, Merton (1973) points to four “norms” through which science functions:

Universalism: science is evaluated regardless of the source or the background of the scientist; Communalism: science should be shared freely within the scientific community; Disinterestedness: science should not be done for the personal advancement of the scientist or for political reasons; and Organised Scepticism: science keeps an open mind, and evaluates each idea on its own merits.

The requirement that I heard many times in my interviews, that science is proper science only when it has been peer-reviewed, also reflects the reasoning behind Merton’s norms, as do the frequently voiced expectations of scientists to be objective or open-minded. Mertonian norms are not to be seen as an alternative to the philosophies of science described above.

A different, more philosophically minded view of science that takes into account the social and historical realities of science is that of Thomas Kuhn (Kuhn 1962) (also discussed in more detail in chapter 5), which argues that science progresses through a series of revolutions intersecting long periods of “normal science”. In normal science, scientists work within a general framework of paradigm which defines the problems of science and the accepted ways of solving them. Disconfirming evidence will be accommodated within that paradigm as anomalies, however anomalies can add up to such an extent that a crisis occurs and people start looking at alternative paradigms.

#### 2.4. *The development of philosophy of science in France*

Although key French philosophers such as the conventionalists Pierre Duhem (1991 [1904]) and Henri Poincaré (1905 [2001]) feature heavily in the literature of logical positivism and the later English-speaking philosophies of science that derived from it, subsequent developments in French philosophy of science have been largely ignored by that tradition. Yet French epistemology (the term *épistémologie* often applies to philosophy of science in general is not quite as narrowly focussed on fundamental problems as is *epistemology* in the English sense) has in some ways presaged developments that occurred in the English speaking tradition after the heyday of positivism. Likewise, English philosophy of science has by now largely replaced the French tradition, and therefore the difference between French and English philosophy of science is these days less pronounced than it had been in the 1970s. Nevertheless, French philosophy of science textbooks still teach the great French epistemologists alongside Popper, Kuhn and the positivists (Andler et al, 2002), while they barely get a footnote in English language textbooks.<sup>11</sup>

Bitbol and Gayon (2006) describe French epistemology thus (my translation):

French epistemology is the name of a tradition of thought deliberately heterogeneous, which has always affirmed if not the theoretical union then at least the solidarity between problems which other traditions tended by contrast to dissociate. (Bitbol and Gayon 2006 p. 2)<sup>12</sup>

These problems include not only logic as in the positivist tradition, but also history of science, and therefore the later development of English-language philosophy of science by Quine and Kuhn, Bitbol and Gayon argue, was influenced by the French tradition.

One of the big names in that tradition is Gaston Bachelard (1884 - 1962). Bachelard's philosophy was contemporary to the logical positivism of the Vienna

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<sup>11</sup> There is no mention of either Bachelard or Canguilhem in Gillies 1993, Chalmers 1982, Ladyman 2001 or Bird 1998.

<sup>12</sup> L'épistémologie française est le nom d'une tradition de pensée délibérément hétéroclite qui a toujours affirmé, sinon l'unité théorique, du moins la solidarité de problèmes que d'autres traditions tendent souvent, au contraire, à dissocier.

Circle and in many ways his philosophy was similar to that of Kuhn<sup>13</sup>, as he proposed the notion of epistemic ruptures which have been compared to Kuhn's revolutions (see Bachelard 2002 [1947], 2003 [1934]; Tiles 2005, Gutting 2001 for an introduction to French philosophy). While Bachelard focussed on the physical sciences, his student Georges Canguilhem (see Canguilhem 2000 for a collection of his work; Rheinberger 2005) looked at the biological and medical sciences. Alexandre Koyre, who himself was an important influence on Kuhn (Hoyningen-Huene 1993), worked more from the perspective of history of science, and at least among historians gets quoted often in the English speaking world. Canguilhem in turn influenced Michel Foucault, who became famous more peripherally to the philosophy of science through his focus on topics such as the philosophy and history of the psychological sciences and the history of ideas (Foucault 1994 [1973], 2002 [1972]).

There were also important philosophers who tried to bridge the gap between the traditions or who tried to popularise the English tradition of philosophical thoughts in France, such as Jacques Monod who wrote the preface for the first translation of Popper's work (Monod 2007 [1972]).

### **3. Science defined in the books**

Almost every single popular science book read for the sample has at least one comment, phrase or even paragraph on what is science, or scientific method. Popular science books are often an account of what scientists have found out. It is natural to accompany these accounts by outlining *how* things were found out to work like that, and why that way of finding things out was so persuasive. One kind of comment is that of a more general philosophical discussion on what counts as good evidence, when to abandon or adopt a theory or hypothesis, or what sets science apart from other endeavours where knowledge is not so secure, such as (according to the tastes of the author) sociology, psychology or theology, and also what sets it apart from fraudulent, pseudo or fantastic science. These accounts are usually longer (in some cases up to chapter or even book length), often feature specific pet philosophers (usually Popper and Kuhn) or scientific methodologies

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<sup>13</sup> There is some evidence that Kuhn was influenced by Bachelard, and that they may even have met, see Castela-Lawless 2004

(such as falsificationism, reductionism or Occam's razor), and some of them will therefore be discussed in the following chapters.

There were also some book length treatments on philosophical topics, like Mayr (1997), Deutsch (1997) or even Wilson (1998); these books will naturally feature quite heavily in the following chapters. These books are generally aimed to explain philosophy of science itself rather than using it to explain a particular part or episode of science. Sometimes though, these books can also see themselves as arguing for a controversial topic, especially Wilson, whose concept of "consilience" was generally not well received neither by scientists nor philosophers (see Segerstråle 2000, Ceccarelli 2001, and chapter 7). Most often however, comments on the nature of science are short comments directed at one particular event, fact or theory in science, which will be discussed in more length in the next subsection.

### *3.1. Philosophical asides*

The most pervasive category of philosophical comments is the smaller aside that can appear in almost any context in popular science books. They are generally very short and in a way merely shift the appeal to authority that any presentation of facts rely on in popular science, to an appeal to authority that "this is just how things work in science", i.e. about the scientific method. Instead of *explaining* how science works, they *tell* us how science works. Nieman (2000, see also chapter 2, section 3.3) analyses similar comments in his characterisation of the uses of philosophical remarks in popular science. Nieman discusses what he calls "pithy" philosophical definitions that scientists give in their popular accounts as a kind of boundary work, and that in popular science "discourse on the meaning of science is more concerned with the defending or capturing of territory than exploring the metaphysical subtleties of knowledge about nature" (Nieman 2000 pp.167-168). Concentrating mainly on two examples, Weinberg (1993) and Davies and Brown (1986), he argues that "popularisations are used as a forum to negotiate authority over philosophical questions" (p. 168). Even though the books that Nieman used are in many ways unrepresentative of popular science in general as he focused only on physics (though his small sample size is remedied in a way because Davies and Brown's book is in fact a collection of interviews with several scientists), his conclusion is essentially valid, based on the impressions from my own sample.

Without mentioning specific philosophers, the type of comments occurred very often where something is shown to be good science because it conforms to the norms of scientific method, whatever the author thinks that is: it is falsifiable, verifiable, it predicts things, is based on meticulous or rigorous testing, observing, peer reviewing. The mechanisms of that method are not discussed further, it suffices to show that the science in question conformed (or the pseudo-science failed to conform) to what the author holds as good scientific practice. I will show in more detail some of these kinds of comments in the following chapters when they concern the topics discussed then, for example when an author wanted to show something as not being science because it fails Occam's razor.

To take a book I will otherwise not much quote from because of its general lack of theoretical discussion, a typical example is taken from Sapolsky's (2001) autobiographical account of his fieldwork observing a group of baboons in Kenya. In a rare exposition of scientific theory, Sapolsky discusses a sociobiological hypothesis about the value of kidnapping in baboon society.

The alpha male is about to pound you [i.e. a threatened baboon]. You don't grab just any kid, you grab someone who he thinks is *his* kid. Mess with me and your kid gets it. Kidnapping, hostage taking. Pretty clever. The idea generated all sorts of predictions. (Sapolsky 2001 p.100)

The crucial part here is that the idea generated predictions, which is one of the most frequently heard attributes of a good hypothesis, and Sapolsky goes on to argue that these predictions were then put towards the evidence to see if they supported the sociobiological hypothesis:

The sociobiological model has been supported only to some extent by the data. Appendices have been added on to the theory. [...] The debate rages on, keeping primatologists off the dole. (Sapolsky 2001 p.100)

Sapolsky here reveals a hypothetico-deductive stance, where it is important for a hypothesis to predict things which will then in turn be checked by experiment or

observation. Sapolsky finishes this section by admitting that the available data has not yet been able to settle the dispute, with a humorous but resigned verbal shrug.

Reminding us that the facts talked about in a popular science book have been the result of painstaking labour and of thinking in a scientific frame of mind, was something that Dawkins for example thought worth pointing out, in a comment which points out that there is a “scientific attitude of mind”, but does not actually tell us what it is, other than that is “meticulous” and “ingenious”.

The facts that I have so briefly recounted are the product of many man-years of meticulous and ingenious work: work that deserves the accolade 'scientific', not because it employed elaborate or expensive apparatus but because it was disciplined by a certain attitude of mind. (Dawkins 1997 p.279)

Discussing one of the staple examples in popular science, especially in the evolutionary psychology/sociobiology literature, Pinker makes this brief assertion about Napoleon Chagnon's (1974) anthropological work on the Yanomamö tribe: “Chagnon's main empirical claims have met the gold standard of science: independent replication” (Pinker 2002 p.117). Here Pinker alludes to another often mentioned norm for good science, that it is replicable, and that the facts accepted by science are not only replicable, but *have* been replicated.

These three examples were taken almost at random from my coding frame to give a flavour of the shortness and often the “pithiness” of the type of comment discussed here. The author was describing facts about the science immediately before and after these comments, so they can hardly be said to be philosophical reflections on the nature of science; rather they are asides, small rhetorical devices that give the reader a quick reason on why something was worth discussing in a popular science book. In two of these cases the reassurance that the science in question is bona-fide methodologically justified science makes sense: both sociobiology and Chagnon's work on the Yanomamö have come under severe criticism, and conceivably need to be defended. (See chapter 7 for the case of sociobiology. Chagnon's work was in fact also fairly contentious in the debates surrounding sociobiology). This is not to say that the author necessarily accepts the

controversial bit, as shown by Sapolsky, who thinks the evidence is inconclusive, but at least they seem to be defending the right of these theories to be discussed as proper science.

Dawkins' remark is somewhat different here, as it appears in the middle of a lengthy chapter which was rich on detail, concerning relatively uncontroversial science (the interaction of fig trees and wasps). The rhetorical reason for Dawkins' insistence that this is proper science, is probably more to do with the general thrust of his book, as it is part of Dawkins' general argument against creationism (see also his earlier book, Dawkins 1986), so that for Dawkins' rhetorical purposes, the whole of biological science has become controversial and to be defended.

There are many exceptions to this rough characterisation though. The short philosophical aside does not necessarily always have to defend a controversial subject as science, and can crop up in very diverse and sometimes surprisingly random passages. On the other hand, the philosophical boundary drawing is not always a short aside either. Jared Diamond, for example, a biological scientist by training whose book touches on many subjects in the fields of history, includes a very long discussion on whether history can possibly be said to be science.

### *3.2. The different implicit philosophies*

I have put the section on the philosophical comments in the books above the probably more interesting section of what the authors have actually written about the nature of science, because it demonstrates the environment into which these short and concise comments fall, and that they do not necessarily add up to a coherent philosophical view due to their condensed nature. The ideas on scientific method that were revealed in that manner often fulfilled a rhetorical requirement (for boundary work) to show up a *specific* episode of science as scientific, and therefore the actual philosophical point may have been chosen to show that bit of science in a most favourable light, rather than necessarily reflect what the author thinks is the most important aspect of science.

#### *Science is hypothetico-deductive*

By far the most frequent assertion about science was that it adhered to some sort of hypothetico-deductive frame. Many authors, for example Sapolsky as quoted above,



have commented on some theory making predictions which in turn have been tested. Like Sapolsky, many of these comments never make it clear whether the testing is looking for falsifications or verifications or both. Other comments merely point towards the desirability of making predictions without further explanation on testing, which is usually, but not always, implied. Steve Pinker for example writes:

Social psychology, the science of how people behave toward one another, is often a mishmash of interesting phenomena that are “explained” by giving them fancy names. Missing is the rich deductive structure of other sciences, in which a few deep principles can generate a wealth of subtle predictions – the kind of theory that scientists praise as “beautiful” or “elegant.” (Pinker, 2002 p.241)

Here Pinker combines the provision that good science predicts lots of things with his ideas of what makes science elegant (see chapter 6 on Occam’s razor). On the other hand, many authors talk about testing and testability as the most important characteristic in science, which implies that there is some prediction on what is to be tested.

Although Popper’s falsificationism is one of the most popular variations of hypothetico-deductivism, in the end it is often impossible to find out whether a scientist follows falsificationism or not. In some cases such a hypothetico-deductive comment has been explicitly accompanied with a remark on verification or falsification. Other authors, though they are not very numerous, have remarked explicitly on something being “verifiable” by experiment:

Nothing would please string theorists more than to proudly present the world with a list of detailed, experimentally testable predictions. Certainly, there is no way to establish that any theory describes our world without subjecting its predictions to experimental verification. (Greene 2000 p.210)

Greene makes this comment in the context of having to defend string theory, the topic of his book, against the accusation many people have made that it is untestable. In the end the suggestion is that it would be great if it were testable, but there are

other redeeming features of string theory that at least temporarily override testability (such as simplicity, see chapter 6).

There was also a curious comment which I could only interpret as saying that some sciences at least are in fact characterized by being *not* testable:

Despite these successes, human imagination is a weak thing. The universe is wilder than we imagine: we keep underestimating how weird it really is. So astronomy is not exactly an experimental science in which the thoughtful predictions of physical theory get tested (Kirshner 2002 p.5)

Instead of being testable, astronomy is unpredictable (see also the comments below on the unpredictability of scientific discovery), exciting, and consistently showing us that the universe is a weird place (which ties in well with Kirshner's position on Occam's razor, see chapter 6).

#### *Science never really proves anything*

The problem of induction illustrated by the famous black swans example, that science can never really prove anything, is also alluded to several times. This is not always merely a hidden falsificationist view, because it has been argued not only for proofs but for falsifications.

Another point about the uncertainty of scientific knowledge does not allude to any inherent unreliability of scientific method, but about the cases when the facts concern things long in the past, or otherwise out of the reach of possible experiment or observation, scientific knowledge is uncertain. Fortey for example talks about the colonization of land by animals, knowledge of which relies entirely on the fortuitous discoveries of fossils, and there is no way of telling if we have ever found all the things that are out there. There may not even exist the important "missing link" type fossils that would put an end to speculation.

Compared to the interviews where such a view was more common, the idea that science is pretty much certain was not very much defended in the books, if we take comments on verification aside. Given that he has in other places commented on the untestability of scientific knowledge, and therefore its uncertainty, Kirshner's

comment that “[s]cientific debates are a sure sign that the data are just not good enough” (Kirshner 2002 p.64), is probably meant jokingly.

One possibly similar sentiment may underline comments that have been made regarding progress. These comments do not argue as such that science finds definite truth, but they express confidence that it *approaches* the truth, or that science gets progressively better all the time. For example, Mayr quotes John Moore’s (1993) “eight criteria for determining whether a certain activity qualifies as science” very approvingly, which may be somewhat surprising given the annoyance he feels about generalized philosophies of science (see chapter 5 below).

Science is characterized by the steady improvement of scientific theories, by the replacement of faulty or incomplete theories, and by the solution of previously puzzling problems. (quoted in Mayr 1997 p.30)

*Science explains the world and it must be consistent*

A completely different type of idea of what distinguishes science is that science is there to explain already known facts (in contrast to the hypothetico-deductivist emphasis on predictions). One such comment was made by Hawking:

Most other scientists, however, accepted the validity of the new quantum laws because of the explanations they gave for a whole range of previously unaccounted-for phenomena and their excellent agreement with observations (Hawking 2001 p.26)

Another such emphasis on the requirement that science needs to explain facts was maybe surprisingly made in the context of UFO sightings, where Webb argues that the popular hypothesis that UFO’s have been seen in Roswell is rejected because

[h]ypotheses are not proven right or wrong through a ballot. No matter how many people believe in the truth of a particular hypothesis, scientists will accept the hypothesis (and then just provisionally) only if it explains many

facts with a minimum of assumptions, if it can withstand vigorous criticism, and if it does not run counter to what is already known. (Webb 2002 p.30)

Here there are also other related criteria for a good scientific hypothesis, it needs to *explain* known facts, but it also must not contradict them. The requirement that good science must be in accordance with what is known from other disciplines, has been made several times when defending orthodox science against pseudo-science, and this indeed seems to be always one of the most convincing arguments against cranks and quacks: in the case of proposed perpetual mobiles, for example, it goes against the laws of thermodynamics: They may conceivably be right, but if they are almost all of the rest of science is wrong. This requirement, that all sciences hang together, was one of the more frequent comments on the nature of science. With its echo of reductionism (see chapter 7), in that it requires a kind of unity of science, it was quite naturally mentioned several times by authors interested in reductionism, for example Wilson. Another good example is Hawking's argument against astrology:

But the real reason most scientists don't believe in astrology is not scientific evidence or the lack of it but because it is not consistent with other theories that have been tested by experiment. (Hawking 2001 p.103)

A similar requirement that "hypotheses must be consistent with the observations and compatible with the general conceptual framework" is incidentally also one of the criteria for spotting science that Mayr quotes from John Moore (Mayr 1997 p.31).

### *Science is unpredictable and exciting*

Sometimes comments try to explain not really what science is as such, but to try to convey the authors' enthusiasm for science, they will emphasise the exciting characteristics of science: science discovers new things and it leads us down unpredictable paths:

Science itself is so often motivated by the will to be first, to attach one's name to a discovery. [...]. A scientific achievement is described as a “break-through”. It allows for the development of a new field, the exploration of a new landscape of discovery. (Fortey 2000 p.154)

This comment tries to show us what motivates scientists and convey the feeling of excitement attached with being the first to discover something. At the same time though, it is also a comment on the more messy human side of science, because far from being the objective and calm enterprise it often projects itself as, science is in fact very much motivated by very human objectives.

Kirshner combines the sense of excitement of discovery with the unpredictability of what we may discover in astronomy:

Astronomy is a science driven by discovery, since the objects we observe are stranger and more exotic than even the most unbridled speculators predict. (Kirshner 2002 pp.5-6)

Naughton sets up the unpredictability of science as a direct contradiction of the popular belief of progress in science, which as shown above, is also held by many scientists. This comment may not come as too much of a surprise given that Naughton has, in the same chapter, been praising the philosophy of Thomas Kuhn (see chapter 5), and this quote is in fact part of his explanation of Kuhn:

We tend to portray it as a record of continuous, cumulative progress. But doing so obscures the fact that at any moment the future is unknowable. What seems obvious in retrospect may not have been at all obvious at the time. (Naughton 1999 p.111)

The mystery and excitement of science is also conveyed by variations on the famous saying that “the more that a scientist knows, the more that he realises he does not know” (Matt Ridley 1999 p.137), and again this can be seen as standing in direct contrast to the authoritative knowledge claims made in popular science books at other places.

### *Science aims at understanding*

There were also more generalized comments that argued not necessarily that science is characterized by a method, but simply that its goal is understanding the world. Like the previous set of comments this emphasizes the excitement of science, but possibly with a more optimistic message that actually portrays the opposite to unpredictability.

The purpose of science, as the late social and political scientist Herbert Simon once put it, "is to find meaningful simplicity in the midst of disorderly complexity." (Buchanan 2002 p.2)

This is also possibly reflected by the “uniformity of nature” type arguments which argue that for science to be possible at all, nature must be comprehensible rather than unpredictable (see also the discussion on what I call the ontological version of Occam’s razor in chapter 6). For example Kirshner argues that “[w]e can decode the universe because the laws of physics discovered on Earth also work in distant places” (Kirshner 2002 p.5).

### *Science is not common sense*

Although the point was not made very often, linked with the issue of unpredictability of science in another way are the types of comment that emphasise the way science fails to conform to *common sense*. This was a very central theme of Wolpert’s famous science war book *The Unnatural Nature of Science* (Wolpert 1992, see chapter 2). Although one of Wolpert’s books did feature on my sample (Wolpert 1999), there he generally stuck to the recounting the facts without explaining much about what he thought about the nature of science. Wolpert’s earlier characterization of science however was quite influential among other popular science authors such as fellow UCL scientist Steve Jones, who himself alludes (without actually referencing) to Wolpert’s thesis. Jones remarks on the intuitiveness of the phlogiston hypothesis, and therefore has to explain why it had to be eventually rejected:

[I]n science, common sense is often a false prophet. It may seem plain that when a fire burns something leaves it to make the flames; but this ‘something’ – phlogiston, as it was once called – does not exist and (whatever intuition might say) instead something else, oxygen, is consumed. The problem lies with what has been called ‘the unnatural nature of science’; that what is obvious is usually not true. (Jones 1997 p.12)

In a slightly shorter aside, Kirshner quips that “Unlike political theory, we don’t hold scientific truths to be self-evident” (Kirshner 2002 p.71), which also suggests that science is not common sense.<sup>14</sup>

*Science is impartial, objective and open minded*

Rather than being a method, science is also often described as a way of thinking: scientists are portrayed as impartial, objective and open-minded. Rather than being a philosophical point on scientific method, this type of comment is more focused on some sort of Mertonian norm, though that still leaves open many philosophical reflections on why exactly holding to Mertonian norms provides reliable knowledge. These reflections are rarely made explicit however, and it is often enough merely commented that scientists are impartial or open minded. Buchanan for example discusses whether history can be said to be properly scientific if it cannot be objective

Does history even work in such a way that “objective” answers to important questions exist? Even historians have expressed their doubts. (Buchanan 2002 p.90)

The requirement of objectivity and impartiality can often also be seen a variant of those comments on the certainty of scientific knowledge discussed above. An extreme example could also be Kirshner’s earlier quoted remark that “Scientific debates are a sure sign that the data are just not good enough” (Kirshner 2002 p.64).

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<sup>14</sup> Although it is not part of my sample, another popular science book argues *for* a connection, however slight, between common sense and science: “Science is something like a third cousin of common sense twice removed” (Pratchett et al. 1999, p.87)

### *Science is replicable*

The quote by Pinker above about Chagnon's work on the Yanomamö has highlighted replicability as a criterion for good science. There were quite a lot of other comments to that effect, for example Wilson puts it first on a small list of desirable features of science:

The diagnostic features of science that distinguish it from pseudo-science are first, repeatability: The same phenomenon is sought again, preferably by independent investigation, and the interpretation given to it is confirmed or discarded by means of novel analysis and experimentation. (Wilson 1998 p.57)

It also features on the list Mayr quotes from Moore (see above). Another example is Naughton's comment on the operating system UNIX, the take up of which enabled scientists "to conduct software 'experiments' which could independently be replicated elsewhere, [which is one of the] hallmarks of pukka 'science'" (Naughton 1999 p.177). Similarly, Fortey tells us that "unlike the cures of Paracelsus, the scientific method deals in repeatability". (Fortey 2000 p.35).

### *Science is actually messier than usually portrayed*

Finally, there are also comments that remind of the "contingent" repertoire (see the discussion on Mulkay and Gilbert chapter 2), where the authors make usually more light-hearted suggestions that science is in fact much messier than it likes to represent itself. It can take the form of a rather maverick scientist complaining that in his case the scientific norms of impartiality and open mindedness were not followed; for example, Horrobin recounts his experience:

Every time I submitted a paper for presentation at a meeting, it was either rejected or given a graveyard slot right at the end of the meeting when almost everyone had gone home. No one wanted to hear what I had to say. No one wanted to give me any money to fund the research I hoped to do. (Horrobin 2001 p.160)



Horrobin's account here is slightly unusual because it is the only book in my sample that has not been an example of mainstream science, and in fact Horrobin's work on mental health has been and continues to be very controversial (see also chapter 1, footnote 3 and chapter 5 on his comments regarding his unorthodoxy).

The sentiment that there is more to science sometimes than objective impartiality is also reflected in comments made by supporters of Kuhn, (see chapter 5), and is also present in Mayr's general argument against normative philosophy. Probably one of the most surprising quotes, given the amount of dislike he inspired during the science wars, was a favourable mention of Feyerabend when the biologist Fortey wanted to emphasise the human dimension in science<sup>15</sup>.

The philosopher Feyerabend has shown that a ruthless mix of ambition, competition and true intellectual gymnastics is what drives scientific advancement. (Fortey 2000 p.247)

One of the best examples of the contingent repertoire as described by Mulkay and Gilbert is a rather light-hearted aside to a comment by Hawking on the hypothetico-deductive method in science.

If the predictions agree with the observations, the theory survives that test, though it can never be proved to be correct. On the other hand, if the observations disagree with the predictions, one has to discard or modify the theory. (At least, that is what is supposed to happen. In practice, people often question the accuracy of the observations and the reliability and moral character of those making the observations.) (Hawking 2001 p.31)

It is interesting to see that at least something resembling the contingent repertoire as described by Gilbert and Mulkay is so well represented in the books, and it possibly attests to the more informal character of popular science where the

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<sup>15</sup> Feyerabend's (1993 [1975]) argument against normative method in the sciences is often taken to be a defence of relativism which much science wars literature sees as a dangerous philosophy (see for example Sokal and Bricmont 1987 pp.78-85)

scientist draws the reader into his/her confidence and somehow manages to convey the idea of speaking off the record.

#### *Other comments*

There were several other comments, which were not greatly represented:

One rather light-hearted remark was to the effect that one of the distinguishing features of science is that it deals with quantifications. “The scientist's job is to take something beautiful and turn it into a graph” (Kirshner 2002 p.18). The idea that science distinguishes itself by dealing with numbers is something that came up in the interviews, but even there not very often, slightly contrary to my initial expectations.

Another comment was an argument that the actual scientific method actually differs greatly between the sciences, which forms a cornerstone of Mayr's criticism of normative philosophies in general (they traditionally concentrate on physics), also discussed briefly in connection with Mayr's views on Popper below (chapter 5). At times Mayr even seems to suggest that what is science depends entirely on the individual: “What one considers science is, of course, a matter of opinion.”(Mayr 1997 p.27). That is of course not really what he is suggesting, given that he also approvingly reproduces Moore's criteria for spotting good science. That is rather meant to emphasise the difficulty that people have had historically in producing a generally accepted definition of science, and accompanies a description of Aristotle's work and why it could possibly be labelled science. The argument that the scientific methods differ between the disciplines is one that I have not read much in the books (only Mayr makes that point), but which I have heard plenty of times in the interviews. There is of course a difference in saying that scientific method depends on the discipline and saying that there are otherwise no discernable things that the different sciences have in common to be called science.

#### **4. Comments on what is science in the interviews**

I usually started the interviews (after asking the respondents a little bit about themselves and their research), by asking “what is science”. Because many respondents hesitated initially, I have also often tried to explain what I mean by that question by asking how the respondent would answer if someone asked “explain to

me why homeopathy/astrology is not science”<sup>16</sup>, or if there is anything that all the different academic disciplines have in common so that they are called science. Sometimes, the initial answers that I have received were comments that said that science is whatever follows the scientific method, in which case I have asked about what the scientific method is. After the initial discussions about what science is, I asked about the respondents’ own education regarding that matter, and what they taught or would teach their own students about the nature of science.

#### *4.1. Initial reactions*

Initial reactions to the question on “what is science” have varied from very confident statements that even mentioned specific philosophers to nervous laughter accompanied with a remark that the respondent has not really ever thought about that, or that they have and come to the conclusion that the question is far too difficult. About ten of the respondents remarked explicitly that it is a difficult question. Some of these have emphasised that they do not know enough about philosophy to comment.

I don’t know too much... it appears to me... it’s not very easy, I find, to define science after all, that’s very, very big, so... [long pause, then gives up]  
(31 Early Career, Physics, Female, France)<sup>17</sup>

This type of comment has usually hidden a little bit of embarrassment that several respondents felt about talking philosophy of science with someone who they might have seen as a philosopher (even though I have always tried to emphasise that I am not a philosopher). A frequent response to my question was to remark that they did not really know, or they were the wrong person to ask because they have never studied philosophy – that was even quite frequently a response to my initial approach by letter. Sometimes I would get a comment to the effect that the question

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<sup>16</sup> I usually used astrology when I was talking to physicists, homeopathy with chemists and creationism with biologists. This presupposed that scientists are all agreed that my typical examples are not science, and I was surprised to learn that was not in fact the case. Below I will show an interesting comment on creationism as science. Also, I have met one biologist who practices astrology in his spare time.

<sup>17</sup> je ne sais pas trop... ça me semble... c’est pas très facile, je trouve, de définir finalement la science, c’est très, très vaste, donc... ..

is a very difficult one followed by a lengthy description of what the respondent thought was science.

There were also some people who were very opinionated about what science is, but who have commented on not being educated in philosophy. On the other hand there were people who had an education on philosophy of science, and used that as the basis of their comment on why the question is so difficult.

#### *4.2. Attitudes towards philosophy, and education received on scientific method*

Scientists' reactions to philosophy specifically were also varied. A lot of respondents commented that they had some formal education themselves in the philosophy of science, while some others told me that they took to educating themselves. Here I am sure there is a large selection bias evident in my sample, because even though I tried not to mention the word philosophy in the approaching letters, people who generally would feel that my topic is unimportant did not respond positively to my initial approach.

However, even a lot of those who did not formally learn about scientific method told me that they think the provisions for learning more deeply about science and its methods are generally inadequate, both in the education they received and in the education that is being given to students at the moment in their own institutions. But this does not necessarily translate into an unqualified endorsement of philosophy of science or epistemology teaching, with philosophy of science often being seen as out of touch with real science, or describing science at best inadequately. Also there were a number of scientists who thought there was nothing wrong with how students learn about scientific method, and although they were clearly in a minority, I believe that due to the selection bias that opinion is much more widespread.

So how do scientists learn about scientific method? Apart from formalised philosophy of science education which does not occur on a regular basis, and which even those that received often thought of as inadequate, scientists learned scientific method often by observation, doing, trial and error. This is reminiscent of Polanyi's idea of "tacit" knowledge (Polanyi 1958, see also Collins 2001), i.e. knowledge that

scientists can only acquire through doing, which they often do not even know they possess, and which therefore cannot be taught in lectures or textbooks.

That kind of education, mimicking one's supervisor, and being acculturated into a scientific community and learning its norms and values only really happened informally and only really started with postgraduate education:

That was something I wanted to say actually about being a PhD student, because then you're starting now to do actual, down to earth, science, you're actually doing practical science. And I think the way that our students learn the scientific method is by example. And early on, you have lot of planning to do, early on, you think which experiments am I going to do, what I'm going to work on, and there's some real feedback there, if a student decides to do something that's not going to work, or isn't consistent with their hypothesis. Then the supervisor will say "you shouldn't do that. That's a bad idea." (23 Early Career, Physics, Male, UK)

This type of comment and the learning-by-doing way of learning scientific method was voiced very often, and it was often acknowledged that it is difficult to teach scientific method any other way. When it comes to picking up the importance of hypothesis testing (i.e. if that is what the respondent thought was important in science, see subsection 4.3 below), then that is of course something that can be taught, but many respondents thought that this does not really translate into acquiring a knowledge of how science really works, which you can only get by doing it.

Nevertheless, often scientists have emphasized that they thought these topics should be taught alongside science in the undergraduate and even in the school curricula. They were mostly the same as those who did attend similar classes themselves in their own education, though because these courses were invariably voluntary, this may not reflect so much that they were persuaded by them about its importance than that they were already inclined to think so. Some even had to go through a bit of trouble to get onto their philosophy courses:

I remember when I was an undergraduate back in [my home country], we actually asked to take an option course in philosophy of science, and we had to actually convince the relevant committee. We did manage to take it in the end, a few of us, but we had to actually argue with the relevant committee that it's relevant to physics, so they said, "oh why don't you take something a bit more solid, something more serious, you know, not just a course in philosophy." (9 Senior, Physics, Male, UK)

Then there were also people who did attend philosophy of science lectures, but came out rather sceptically. On one end of the scale was a respondent (12 Senior, Biology, Male, UK) who was very enthusiastic about the course he attended, and then when he started doing science professionally, decided that science actually works very differently to how that course had taught him. His comment will be quoted at further length in chapter 5 – he told me that he was taught from a very anti-Popperian perspective, but later decided that Popper described science pretty much correctly. On the other end was a theoretical physicist who attended a philosophy of physics lecture and came away with a very bad feeling about philosophy of science and its relevance and even understanding of science:

My memory was that a talk about interpretations in quantum mechanics. And the speaker, he was using all these words, that as a physicist I've been taught what they mean. And he was using them in a slightly different way, and the net effect was, he was getting worried about things that he shouldn't have been worried about... [...] It's a few years, but I remember thinking this guy needs to be sent off to go through all the lecture courses, and then he wouldn't have all these problems. (10 Early Career, Physics/Chemistry, Male, UK)

Though this is not about the philosophy relevant to scientific method, this experience did colour this respondent's expectations of philosophy and its relevance to professional science, although in the subsequent discussion he was more open to philosophy of science than the quote may suggest.

Finally, several times the idea that scientific method does not necessarily need to get taught was mentioned. The argument is not that scientists do not need to understand scientific method, but that the understanding comes naturally to people with the right “scientific mindset”. People who lack the scientific mindset invariably will not have a successful scientific career, so that at the end only people who properly understand scientific method occupy the senior positions in science: “It’s almost like people that pick it up are the ones that get promoted, and keep jobs, and stay in science” (23 Early Career, Physics, Male, UK). Interestingly this can be an almost optimistic position that in effect argues that good scientists are self-selecting, and that through this system the bad scientists are weeded out:

I think it [reflections on the nature of science] just comes up in discussions, and it’s just, you talk to scientists...[...] I think this is what for me in a lab, or in a lecture or whatever separates those that will become scientists from those that will go to companies and become basically technicians, they will still do their PhD probably, they will still be a postdoc or whatever, but very few of them will become scientists. And I mean even among those people that I’ve seen doing their PhD, I would say 98% aren’t scientists. Because they don’t do *that*. (25 Early Career, Biology, Female, UK)

More often however, I have found that the respondents thought that teaching scientific method or the scientific mindset is very important. One respondent even argued that the general lack of philosophical knowledge is the cause of a lot of “disfunctioning science” (36 Senior, Biology, Male, France).

#### *4.3. The philosophical opinions*

Just as with the books, there was a variety of opinions about what distinguishes science from non-science. Although strictly speaking there was no equivalent in the interviews of the philosophical asides in the books because I specifically asked the respondents about their reflections, there were quite a few similarities in the content of the answers. Some interviewees had a ready answer with which they would characterize science. These could range from a quick reply about testability or open mindedness that in many ways echoed the pithy asides in the books, to respondents

who talked at greater length about their preferred idea of scientific method and who clearly have thought about it a lot before, to one respondent who even worked out his own epistemological position, which he has even published himself in philosophy journals.

### *Science is hypothetico-deductive*

The idea that science is characterized by some sort of hypothetico-deductive structure, where scientists construct hypotheses, which will then get tested by experiment or observation was, as was the case with the books, by far the most frequent answer. I will discuss these answers in more length in the next chapter because the scientists' ideas regarding hypothetico-deductivism were very relevant to their opinions on Popper's philosophy, which after all is a variant on that theme. Just as was the case with the books, there were several ways the respondents voiced the idea. Followers of Popper for example, unsurprisingly, emphasised the falsification aspect of the testing, while many others spoke of verifying or even of both verifying and falsifying.

Others emphasised the idea that science predicts phenomena, without necessarily mentioning that those predictions will be put to test, though that was usually implied. Within a sea of comments such as that what distinguishes science is that it is testable, there were also some expressions of disquiet. First of all, even the idea of theories, and research that is driven by hypotheses was questioned:

Respondent: I'm not entirely happy with theories, that, the business of hypothesis driven research, I find a little bit uncomfortable sometimes, because there are other ways of doing good research.

Me: Such as?

Respondent: I like the, the "this is an interesting question, let's [*inaudible*]" approach. "I wonder what would happen if..." (8 Senior, Biology, Female, UK)

The complaint that there is more to science than just theory testing, was heard very often, and will also be discussed more in the next chapter. However, though this reservation was often voiced during the discussion on Popper, some scientists,



like the one quoted above, have even brought this point forward within the initial discussion of what science is. This conscious counter-positioning of course also signals that these scientists know that the dominant scientific discourse on method follows hypothetico-deductivism.

*Science never really proves anything*

This was also a theme that came up both in the interviews and the books. Many respondents replied that science is fundamentally uncertain, and that it could always turn out to be different than you think, because it is impossible to really prove anything. This remark did not usually come up as a first kind of characterization of what science is; it often came at the end of the discussions following my question. There were on the contrary some remarks that characterised science as being certain. For one respondent that certainty of science meant that the more statistical sciences are a bit less scientific, and the social sciences are definitely not really scientific:

I think when there is not a definite yes or no answer then, then pureness of the science starts to disappear. What is [taught as?] science is not right... what it hasn't... Physics is critical with yes or no. Say particle physics, astrophysics, these are statistics, but you can, from the statistics you could make a good yes or no answer. And social sciences, are... they don't have this distinct [*inaudible*] that pure sciences do. (5 Early Career, Physics, Male, UK)

What I heard more often however was the idea that, while science is strictly speaking always a little bit uncertain, because you can never *really* prove or disprove anything, in many cases, especially in cases of science in the media, we are very, very close to being certain. An admission that we can not really prove anything is often seized upon by the media to have a discussion about things where there really should not be any. What is needed, it was argued, was a way to differentiate between when something was debatable and when something was *as good as* proven.

Because I think that's a skill which you can apply in the general public to what they hear about science in the media, and I think it's relevant from the point of view that at the moment the media has a requirement often to provide equal weight to two sides of an argument, but in fact often in science it's 99 per cent one side and one per cent the other. (13 Mid Career, Biology, Male, UK)

*Science explains the world and it must be consistent*

Some comments also defined science as being the activity that tries to explain the world, in a more or less inductive way – we make observations about the world, and the science is there to explain them. Some scientist for example were unhappy about the emphasis on hypothesis testing, which almost everyone thought was important, but neglects the process of coming up with the hypothesis in the first place (see also chapter 5):

I think one thing that people often forget in the science, when thinking about the scientific method, and one thing that certainly I did not discuss much over the last couple of minutes, and that is, is that you can't come up with a hypothesis without the observation. (19 Mid Career, Chemistry, Male, UK)

This is also apparent in this quote, where the respondent was adamant that science is often an inductive process.

My thinking about science is to see what's happening. And then try to accumulate evidence about whether we can, whether we can form an hypothesis. (20 Mid Career, Biology, Male, UK)

Next to this emphasis on induction that was very strong for some of the respondents, there was also the idea that science is there primarily to find out about the world and to explain it. Just as I wrote in the equivalent section on the popular science books, there is also here a relation to the frequently made argument that the sciences have to be consistent, not only with our background knowledge, but also with each other. If the new science contradicts an already established fact, either a

known observational fact, or a very well established theory, then there is a lot of explaining to do, and at the very least the burden of proof rests on the new science. It is this kind of requirement that lets scientists argue against the more fantastic and even pseudo-scientific proposals. I do not mean this to be merely a boundary drawing approach by scientists who delineate who can talk about science authoritatively (see Gieryn 1999, discussed in chapter 3, and the further discussion of this chapter in the next subsection), though I am sure that there is a fair amount of boundary drawing going on here too. Rather, this has been a fundamental philosophical point that has been made, often without specific reference to any particular pseudo-science, although it has also been used to argue against pseudo-science. Sometimes similar reasoning was made to explain that a fact is more established when the evidence for it comes in from completely different sciences, or at least from different experimental approaches.

In general if you want to establish a fact, you have to establish it by more than just one technique, one approach, you want to have several ones, and what's really exciting about physics is, is when theory sort of meets experiment, and it's not very often, I mean, but when you do examine them by a totally different approach you get the same result, well... (40 Senior, Physics, Male, France)

#### *Why we do science: Science is exciting and useful*

When asked the question about what science is, several people told me about the reasons they went into science. Though these are not strictly philosophical points, some respondents felt that its ability to fascinate curious people, or to provide useful applications, was one of science's primary distinctions. Coupled with the sense of personal excitement on science was also a sense that it is useful.

For me science is, is the pursuit of a question that is important, that you're passionately interested in... if you solve it will not just be satisfying to you, I think that's very important, it should be satisfying to you, but it will also be of some benefit. (18 Mid Career, Biology/Physics, Male, UK)

The usefulness criterion was almost exclusively used by scientists whose research had medical applications, probably unsurprisingly not by cosmologists. Also the idea that science is useful did not find much expression in the popular science books, though if you write a book on the biology of cancer the usefulness of science probably goes without saying. Only one respondent who was not involved with medical applications made a similar point. This is from an ecologist who was much concerned about environmentalism and who looked to science, his discipline, for help – not necessarily to find solutions, but at least to give us a better understanding of the natural world so that we can find solutions.

Why I think science is different [from non-science]? Because I think it's the discovery of new facts which can be put to use to improve human life for the future, or to, I know it's an old cliché, change the planet. (11 Mid Career, Biology, Male, UK)

Comments like those in the popular science books that science is unpredictable and therefore exciting surprisingly did not get voiced much in the interviews. Though several comments on unpredictability have been made in other contexts (such as the comments discussed above that nothing can really be proven, see also some of the comments further below on replicability), the explicit tone of excitement that unpredictability gives us in science which were in the books was pretty much absent (except for Kirshner), and the unpredictability of science was seen rather as a nuisance.

#### *Science is not common sense*

Although, just as in the books, comments on this were not made very frequently, there was disagreement on that point. One respondent was very adamant that science is merely a methodologically sound extension of everyday reasoning, and therefore was very unhappy with Wolpert's thesis that science is not common sense when I told her about it.

To me the scientific method is just a formalization of common sense, it's actually... it's a formalization of something that we as human beings do

naturally to find out something, [...]. A child will go through a series of steps to try and find out about the world, and some of them would be based on erroneous premises, but a lot of them will be a child testing things. (15 Mid Career, Chemistry, Female, UK)

Contrasting with that view, there was one scientist who agreed with Wolpert though, that science is not common sense, and the particular argument was that the proper scientific reasoning, accepting the evidence even if it sounds crazy, was something that scientists do, which the “man on the number 9” has difficulty with.<sup>18</sup>

Often common sense suggests that you are trying to prove an experiment correct, but of course if the result demonstrates that it’s untrue, then you accept that result” (7 Senior, Biology, Male, UK)

Coupled with that are probably some of the comments that emphasise that science is difficult – though that on its own says nothing much about its relation to common sense other than maybe suggesting that science is not readily accessible to the man on the street.

#### *Science is impartial, objective, and open-minded*

Especially when contrasted to non- and pseudo-sciences, respondents liked to point towards the objectiveness and open-mindedness of science. Linked with that is the ideal that a scientist will give up his or her favourite theories when confronted with contrary evidence.

what [my chemistry school teacher] used to say, “it is a scientist’s bounded duty to hold his theories lightly and give them up graciously when proven otherwise by somebody more... clever”. Or something like that, I mean that’s the wrong words, but that’s the gist of the quotation”. (8 Senior, Biology, Female, UK)

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<sup>18</sup> “The man in the number nine” refers to a hypothetical intelligent layman as could be encountered on a suburban bus; the idea is more usually expressed as “the man on the Clapham omnibus”.

This is however also often linked to an admission that this really is only an ideal, and that scientists are often human and fallible. The sentiment is of course also linked to some of the other philosophies, especially falsificationism, which stress that scientists should gladly abandon falsified theories, as well as consider other theories, even wacky sounding ones with an open mind.

Next to being impartial and open minded, many people found it to be a defining characteristic of science that it should be hard, rigorous and precise. These comments, even more so than the open-mindedness ones above, seem themselves quite imprecise, because it never really is quite clear what precise or rigorous actually is, and following up the discussion then turns to the other philosophical criteria discussed in this chapter. However, it is worthwhile mentioning here that these qualities are so often seen as defining science, even apparently on their own.

### *Science is replicable*

If science has found out a universal law in an experiment, then it should be expected that subsequent experiments lead to the same conclusion, and that the conclusion is even more valid if this replication is done with different method, or even using a theoretical framework from another area, which reflects the argument that this would rule out that the result arises out of an error in the original experimental design. Although philosophically this insight is similar to the one discussed above about science having to be consistent with our background knowledge and with other sciences, the replicability of scientific results was mentioned explicitly so often in the interviews that it stood apart as one of the hallmarks of science.

Replicability can sometimes come up as an aside when the respondent wanted to make a comment on making experiments:

The thing that makes it a science is that you have, you come up with an idea, you test that idea in a way which is repeatable... (2 Early Career, Physics, Male, UK)

Or it is part of the process of verification and falsification itself, because everybody should be able to verify or falsify a scientific finding (34 Mid Career, Physics/Chem, Male, France). But it is also seen, apart from testing, as a defining characteristic of

science in its own right, something that science always does, or rather should do, to be scientific. It is recognised though that most experiments are never actually repeated, which is something that some respondents have also complained about:

this brings me on to something in science, which I think scientists don't do anywhere enough of, which is repeatability (19 Mid Career, Chemistry, Male, UK)

But even though most experiments are never actually repeated, it was also mentioned that this is mitigated by a natural scientific practice where the most important and surprising results are always the one percent of experiments that get repeated. (15 Mid Career, Chemistry, Female, UK).

However, even in this area there was disagreement. One scientist (14 Mid Career, Biology, Male, UK), who researched the water ecology in lakes told me that his results are *never* replicable. If I read his paper and decide to go to the lake in question and perform the same study, the lake will inevitably have changed in a significant way, and even if it has not there will never be any possible way to tell if the study really was repeated, because in a natural environment we will never be able control for all the important variables.

### *Differences in method*

One point on which there was substantial difference between the opinions as expressed in the interviews and in the books was that many interviewees told me that things are different in the different sciences. There is a range of comments here: Most are asides that qualify a definition of science by saying that he/she can of course only authoritatively talk about their own experience in their own field. They are often made in response to a specific philosophy I introduced in the interview. For example on Popper, one response was that it might have worked like that in the science Popper was familiar with, but not in her experience (8 Senior, Biology, Female, UK).

But there was also the expressed opinion that the methods of the different sciences vary, and in one case it was even argued that scientific method varies between individual scientists. The complaint by Mayr quoted above, that

philosophers concentrate too much on physics was also echoed, so that here we have a comment that philosophy is slightly out of touch with real science which is justified by the contention that the sciences are different in their method.

I would really like to see, to read, you may know... I'll be there, you know a sort of critique of the philosophy of science as applied to more hands-on, wet, science. (17 Mid Career, Chemistry, Male, UK)

This was however not always backed with personal experience by some of those scientists who had switched disciplines. The following answer to my question of "what is scientific method" was made by a trained physicist who got into medical physics, and then found herself directing a medical laboratory.

That's weird, because... me, I have... before, when I did nothing but physics, I've always been under the impression that the method should very much be linked to the field that you worked with, whether you do physics, chemistry... But now that I am in a neuroscience lab and that I work with psychologists, you could say that's not the sort of science you'd call "hard", I find that at the end the reasoning is the same. (35 Senior, Biology/Physics, Female, France)<sup>19</sup>

But even she later made several comments to the effect that, while the overall reasoning was the same all over science, there are subtle differences in the actual methods: "Psychologists, they don't have equations, but they have models".<sup>20</sup>

### *Science is inductive, or a collection of facts*

There were also comments that emphasised the accumulation of facts in science. People who made these remarks always pointed out that science should of course

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<sup>19</sup> C'est marrant, parce que... moi j'ai... avant, quand je faisais que de la physique j'avais l'impression que la méthode devait être très liée à la [...] matière qu'on fait, si on fait de la physique, de la chimie, de ça... Mais maintenant que je suis dans un lab. neuroscience et que je travaille avec des psychologues, on pourrait dire c'est pas de la science qu'on appelle « dure », je trouve que finalement les raisonnements sont les mêmes.

<sup>20</sup> les psychologues, ils ont pas des équations, mais ils ont des modèles.



not only consist of collecting facts, but that a lot of characterisations of science miss out on this rather fundamental aspect. Therefore, for some scientists, induction had an important role to play in science. For example one biologist argued that an important part of science was laborious fact finding which has to be done before hypotheses can be constructed at all.

I think this aspect of science also this kind of accumulative... accumulation of limited, but somehow useful, knowledge is an important part of science. (36 Senior, Biology, Male, France)

Very close to the idea that science is about collecting facts, is the inductive argument that science arrives at conclusions by generalising from the facts it observes:

That, too is a... thing common to all scientists, that from a particular thing we try to get a generality out, I think. (29 Early Career, Physics, Male, France)<sup>21</sup>

Because the argument against induction was so central to Popper's philosophy, this more inductive side of science will be further discussed below in chapter 5. Here I would like to point out that people who argued for science being the accumulation of facts never argued that that is *all* science is. In this sense they were then also quite close in opinion, though not emphasis, to those who pointed out that science is *not merely* the collection of facts, but also has other qualities, which was a point made more often in the books than in the interviews.

### *Science is more messy*

Next to the comments on the rationality of science and scientific method, there were also plenty of admissions that real life science is usually much messier than that. There was a general feeling that the human side of science is very much an inseparable part of science, whether you think it is a good thing, or whether you

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<sup>21</sup> ça aussi c'est une... chose commune entre tous les scientifiques, c'est que d'un truc particulier, on essaye de sortir une généralité, je pense.

think it is regrettable but unavoidable (comments on the Mertonian style social norms of science such as open-mindedness were so frequent that they are discussed in a separate section above). I will discuss most of these kinds of sentiments in the next chapter, because I would argue that Kuhn's philosophy at least in its popular perception is precisely about contrasting the logical, inhuman and machine like characterizations of science with a more realistic portrayal of how scientists actually work, warts and all. Here I will give only a couple of quotes that I feel were genuinely meant more to be tongue in cheek, or contrary to how that particular scientist actually characterised science, which I think is closer to the actual contingent repertoire as identified by Mulkay and Gilbert. The explanation of how the messy side of science works within a generally empiricist view of science was sometimes expressed in a "truth will out" manner, as found by Mulkay and Gilbert:

you get opposing camps who essentially block each other in various ways, so all that's social, but ultimately [...] I think that the social things can't get in the way of truth (12 Senior, Biology, Male, UK)

They could also be, admittedly in a more tongue-in-cheek manner, refer to the way that the social working of science itself produces the truth. One such comment, which may have disguised some cynical bitterness about the process, and otherwise did not represent how this scientist feels science ought to work, was made in response to my question about at what stage a scientific finding would become a generally acknowledged fact. This may possibly correspond to the way the scientists in Mulkay and Gilbert's study have used humour as a device to marry the contingent and the empiricist repertoires. This respondent was maybe cynical, but certainly quite serious about the point she wanted to make:

then what happens is then *your* paper then gets quoted as "[respondent 24] said this". And then it becomes a fact, whereas... I mean a guy I worked with, he said that he made some very off-hand comment in a paper about how an enzyme worked, and for the last ten years, everyone has put in their papers that this whole system is based on how this enzyme worked, by some throw-away line that he put in his paper, that he had no evidence for, didn't

claim to have any evidence for, but once you've written it down, somebody can then quote you as saying... and once it's quoted, you have no responsibility, so you can just use that as fact. And that's what happens. (24 Mid/Early Career, Biology, Female, UK)

However, many respondents felt that the social, messy, side of science is a necessary part of it, and that even things like the personal attachment scientists have for their pet theories is in some circumstances good thing, rather than always hindering progress as Popper would have argued:

scientists are particularly prone to getting attached to something which objectively they might not do. And I think this is because as a scientist you need to have a good intuitive feel, and that's important for inventing hypotheses. And so, you learn to have maybe too much confidence in your intuition, which can get into the way of being objective. (23 Early Career, Physics, Male, UK)

More comments on the social side of science were made in the context of discussing Kuhn's philosophy and will be presented in the next chapter. In these discussions the social sides of science were very often seen as necessary parts of science.

### *Other comments*

Next to the sharp demarcation between science and non-science or even pseudo-science, an attitude which I have to admit I have taken pretty much for granted, there was also a curious comment regarding a continuum between science and non-science, where there is a lot of grey area between things that are definitely science and things that definitely are not.

I know that what is currently seen as a scientific fact is only as well understood as the existing theories that support it are good and useful, and that therefore there is an element of belief attached to most scientific so-called facts, however, there is a difference in strength I think between the evidence and the theoretical underpinnings of some areas versus others, and

I think that there comes a point... there probably is a grey area, but there is some things which fall into the one side of the grey area, and some things which fall into the other side, and homeopathy for example falls to one side, and shall we say structural biology, falls in the other. (20 Mid Career, Biology, Male, UK)

Next to there being a continuum between science and non-science, another surprising idea I heard was to admit something as science which most scientists, especially from this discipline, thought of as non- or pseudo-science. When I asked this ecologist about how he would justify the belief that creationism is not science I got an answer which rather surprised me.

I don't justify it, because I wouldn't deny it because... I have no evidence that it's wrong. And so, when we teach evolution to our students, we allow them, if they're interested, and are... if they are completely persuaded by creationist theory rather than evolution by natural selection, that's absolutely fine. It's not for us to change their view. Because I've never seen a paper which completely disproves creationist theory. It doesn't exist. (11 Mid Career, Biology, Male, UK)

This remark is completely in keeping with the frequently discussed comments shown above about the fact that nothing can ever really be proven or, despite Popper, disproved. That did not mean that this scientist *accepted* creationism in any way – he merely thought that there were other reasons not to believe in it, such as its implausibility or it running counter to other very well accepted scientific theories. In other words, he thought creationism *is* science, but not accepted science, which is a perfectly consistent position even for someone who then continued to explain in detail why he thought creationism was wrong.

Another interesting idea which only came up once in the interviews when I asked to contrast science with things that are not science was that science answers “how” questions, not “why” questions (10 Early Career, Physics/Chemistry, Male, UK). An interesting thing here is to note that when I asked about what is science, this respondent did not think about contrasting science to pseudo-science, but to

contrast it to theology and philosophy, both of which (with some reservations about philosophy of physics, see his comments above in section 4.2), as a keen Christian he thought were worthwhile pursuits.

## **5. Conclusions and summary**

Between the books and the interviews, there was often a difference in the type of the comments and the reasons why they were made, because they were made in completely different contexts. There is no real equivalent to the philosophical aside on scientific method or the nature of science in the interviews because I specifically asked for people's opinions in these matters. In the following three chapters where I will discuss reactions to specific philosophers and philosophical concepts and ideas, a comparison will be much easier because I will focus on comments made on a specific topic in the books, on which I then asked questions in the interviews.

In this chapter, I have deliberately focussed on providing a broad summary of the different philosophical opinions found in the books and the interviews, as well as a brief survey of the interviewees' more general attitudes towards philosophy and the education they themselves received on the scientific method. Next to being interesting in its own right, this provides a philosophical background to the following chapters. In this subsection however, I will look briefly at some of the themes this chapter has thrown up: First I will briefly discuss the idea of the philosophical remarks in the light of boundary work, (and also Gilbert and Mulkay's concept of interpretative repertoires), putting this chapter into the theoretical framework for this thesis, as outlined in chapter 3. This theme will be explored more deeply in the following chapters where the in-depth discussion of single philosophical topics will make the analysis more compelling. I will then explore the idea that the philosophical thoughts of scientists can best be divided into three distinct philosophies, which despite the way they are often discussed in the philosophical literature, are not seen by the scientists as mutually exclusive. Finally, I will end by highlighting some of the differences, as well as the possibly surprising similarities of philosophical opinion between the France and UK-based scientists.

### *5.1. Boundaries and repertoires*

In most books, the comments on the nature of science were only peripheral to the overall argument, which after all was usually about explaining the science rather than how science works. The role the philosophical comments often fulfil I have argued is in legitimating the scientific theory or episode as properly scientific – this may be because what is under discussion is or has been in some way controversial, or because the author writes the whole book at an audience he/she thinks may be sceptical. In either case I imagine that these comments reveal a little bit about the scientists' idea about scientific method, even if they invariably never add up to a consistent philosophy, and may even run the danger of incoherence at times, as possibly demonstrated by Kirshner.

There may be some comments to be made concerning the empiricist and the contingent repertoires as identified by Gilbert and Mulkay (1984, see chapter 2), as I do not think that there was too much evidence of a division between interpretative repertoires for the scientists to draw on. There were plenty of comments that drew from the discursive resources that Gilbert and Mulkay identified with the contingent repertoire, which interprets science as not infallible, objective and machine-like as it likes to present itself in scientific articles, and I have tried to point to those in the sections above, especially the ones on the “messy” side of science, where people talk more about the human and irrational side of science. These however were not divorced from the pure empiricist repertoire about science, insofar as it existed at all, because the human side of science was also often acknowledged as an inseparable part of science. Consequently the contingent comments show scientists' general knowledge that science is not all that rational all the time. Therefore I have on only four occasions in the interviews heard phrases that remind of the “truth will out device” that Gilbert and Mulkay point to as a coping device between the empiricist certainty of science and the contingent knowledge that science has human fallibilities. So in a sense while there was some evidence of the two different repertoires, I have not found the division to be particularly strong. Mostly the scientists knew that real life science has different characteristics, some of them human, and they are very ready to admit this, in private conversation as well as on the record, at the same time as they engage in a description of the empirical nature of science. Ultimately, just like professional sociologists and philosophers of

science, the scientists in the study were well aware of both sides of science, and mostly held sophisticated views of science which take account of them both, going beyond the naive-sounding trust that the truth will eventually be found.

For the interviews at least this may be a result of me specifically asking the respondent (and looking out for in the books) about their idea of the nature of science, rather than it coming as a by-product of a discussion about their actual science, as it does in Gilbert and Mulkay's study. But in any case, for that reason I have found an analysis on the respondents' empiricist and contingent repertoires to be somewhat unfruitful. Instead, building on the demarcational nature of the philosophical asides in the books, and the fact that I have specifically asked the scientists to describe what the distinguishing features of science are, I will concentrate on the possible boundary work performed by the scientists on what is and is not science.

The authors' frequently short but concise efforts to explain what science is and is not, can be seen as an almost classical example of boundary work in the sense of Gieryn (see chapter 3). This conclusion was also reached by Nieman in his analysis of the pithy remarks on the nature of science he found in the popular physics books he analysed. As I have outlined above, the philosophical remarks often functioned to either justify something as properly scientific, or to show that something does not have much merit because it is unscientific. This would typically occur when the science to be defended was acknowledged by the author to be controversial, such as sociobiology or Chagnon's anthropological work. Or, as in case of Dawkins, the author may address a hypothetical readership that thinks a piece of ordinary science (evolutionary theory) is controversial. In both cases the author draws a boundary around what he thinks is acceptable science. In other cases, such as some of the remarks on Occam's razor that will be discussed in chapter 6, the boundary work consists of excluding things that are seen as definitely not science at all.

In the interviews, the demarcation aspect of the philosophical remark did not come across as clearly, because I have asked the question "what is science" directly. However, it is interesting to note how quite a few interviewees have interpreted that question, as they then contrasted science not only with things they thought were not

science, but with things they disapproved of: astrology, creationism, homeopathy. Only rarely did a respondent contrast their definition of science with something that (for them) is not science, but still a worthwhile pursuit, such as philosophy, social science or theology. Sometimes, when the interviewee struggled initially with answering my question, I would rephrase it by specifically contrasting it with what they thought is not science, I found it interesting to note therefore that as soon as the question was framed as a demarcation question, interviewees found answering “what is science” much easier. The case for the scientists’ philosophical remarks being examples of boundary work (as well as expressions of disciplinary identity) will be further explored in the following chapters (especially chapter 7 on reductionism), where the individual topics will be looked at in more detail.

### *5.2. Three types of opinions on science and scientific method*

The most common response by far, both in the books and in the interviews, was the idea that science is hypothetico-deductive, in the sense that it is about formulating hypotheses, retrieving predictions from them, and testing them in experiments or observations. What shape these tests take, and whether we are specifically looking for verifications or falsifications varies, and is probably seen by many scientists as much less important as the debate amongst philosophers has suggested. Hypothetico-deductivism, and in particular the issue of falsification versus verification will be looked at in more detail in the next chapter. Another point where comments on hypothetico-deductivism varied was in the emphasis either on predictions or testing – some people thought that the most important characteristic of science was that it predicts things, which of course implies that we also check those predictions, while others thought the experimental testing was the most important thing which likewise implies that there was a theory or hypothesis that told us what to test.

Somewhat different in their philosophical message were comments that would probably be better regarded as inductive – where the most important aspect of science is the accumulation of facts, or evidence. Also important was the idea that the hypotheses need to get generated in the first place, and for that we draw out whatever we can see from the accumulated evidence. In the same category I would also put the comments on science being primarily about generalisations. Inductive



comments were much more rarely made in the books, which is quite curious, possibly because the books address a lay audience and the authors may feel that the public perception of science is one of the accumulation of facts anyway and they rather need to be convinced of the other aspects of science. In any case the samples are probably too small to make such a conclusion convincingly.

Then there were also comments that emphasized the social side of science. As already argued, the comments on the social side of science were not at all always negative and included ideas on the social norms of science such as those introduced by Merton: More narrowly defined institutional norms such as peer review, and comments regarding the inevitable influence of fickle human nature, both negative, such as the citations-make-facts comments, as well as the positive, such as scientific competitiveness driving the pace of discovery. These types of comments, especially those regarding revolutions in science will likewise be discussed in further detail in the next chapter (chapter 5).

As I have hinted at in the philosophical introduction, these topics are not necessarily the way philosophers themselves divide their subject into. On the one hand, hypothetico-deductivism is such a broad area that its several different varieties, such as falsificationism, positivism or Bayesianism were and still are fiercely contested, whereas for the scientists in my study the most important aspect of scientific method was the basis they all shared and the details were mostly less important. On the other hand, the strict division that philosophers have drawn between the empiricist/logical aspects of science and its social sides, often with the approval of science warriors, does not seem to be held widely by the scientists. Like the subjects in Gilbert and Mulkay's study, they often talked about science in a "contingent" way, however (maybe because I asked them directly how that fitted in with their other views) I have heard very often that these two sides of science are both necessary for it to function.

### *5.3. French and English conceptions of science*

One of the interesting results from the interviews was the similarity between the UK scientists, the French scientists and the three scientists who worked in both English and French speaking countries (see chapter 1). Even though the philosophical development of epistemology and philosophy of science in France and in the

English speaking world has taken completely different routes, opinions on the nature of science were relatively similar. The general level of interest in philosophy was also very similar. A minority of interviewees in France and the UK had a lot of interest in philosophy, while most were politely interested but did not know a lot about it, and some were indifferent. Nobody was actually hostile towards philosophy.

The similarities were even more pronounced with the younger scientists. When I occasionally did hear some comments concerning the uniquely French epistemological tradition, these invariably came from older and very established scientists. However, even among those, I would count only one (37 Senior, Biology, Male France) as actually *following* a French philosopher of science (Bachelard), and even then he saw a lot of that philosophy in terms of the English tradition (Kuhn, in this case, see also chapter 5, sect.4.3).

Another of those established scientists (who was quoted above as being himself very interested in developing his own philosophy of science, 39 Senior, Biology, Male, France) even told me that he thought the French philosophical tradition has “almost disappeared now”. Similar to some of the English scientists, this interviewee has himself read a lot of philosophy during his early scientific career, and unlike the English speaking scientists, his reading list did feature a lot of the French epistemological tradition (as well as a lot of Greek philosophy). However, it also included the main works of Anglophone philosophy, and I found no-one in France who was familiar with French philosophy of science who did not also know plenty about Popper or Kuhn.

Popper did have an impact on French scientific culture, if not necessarily the philosophical culture. It was slow to take off, with the first French translation of the *Logic of Scientific Discovery* appearing only in 1973, endorsed however by a preface of one of the most important French philosopher/scientists at the time, Jacques Monod, which did influence at least one respondent to take Popper seriously:

I guess it was also through Jacques Monod and the French school of molecular biology that I discovered Popper. Because Monod really

considered that it was *the* philosophy of science. (36 Senior, Biology, Male, France)

One very interesting observation is that even though among actual opinions on the nature of science I found no particular difference between France and the UK, and even though most of the respondents' level of interest and knowledge of academic philosophy of science was pretty much the same, some told me that nevertheless French culture and specifically French scientific culture was much more philosophically oriented – in the two cases where that opinion was voiced most forcefully, the respondent actually had personal experience of working in both a French and in an English speaking university (32 Senior, Physics, Male, France/En and 26 Early Career, Biology, Female, UK/Fr – the first is a English-speaking scientist who has by now spend most of his career in France, the second is a recent French PhD graduate working as a postdoc in the UK). On the other hand a Francophone scientist working in the UK (16 Mid Career, Physics, Male, UK/Fr) told me that he perceived no difference.

Concerning French popular science books, I have not seen any difference either. Although I originally planned to also read some French popular science books, I had difficulties constructing a sample for French popular science books, especially because most of the most influential popular science books sold in France are (and have been for a long time) translations of the English classics, such as those of Dawkins, Hawking and Gould. In the absence of a similar criterion as the Aventis prize and no available sales figures, this is based on my own observation of browsing French bookshops, and I was also told this when I asked some of the French interviewees about their impressions of popular science books in France. Insofar as popular science books may have an influence on the thinking of later generations of scientists, English books will be read just as much as French books, which supports the suspicion that France's (and everywhere else's) scientific culture is getting more and more Anglophone with the spread of English as the scientific language. An illustration of the fact that the spread of English as the most important academic language has also spread to popular science books, is that one of the authors in my sample, Gerd Gigerenzer, is in fact German and based in Germany,

yet chose to write his book directly in English and then had it translated to German afterwards.

I have nevertheless found a few genuinely French popular science books (for example Luminet 2001) that have been popular in France. Also, two of the French scientists interviewed have written fairly successful popular science books themselves (37 Senior, Biology, Male France and 39 Senior, Biology, Male, France). Nevertheless, even though I found that both these scientists were much more than averagely interested in philosophy of science (see above) their popular science books were philosophically comparable to the English ones in my sample and to the other French books read.

#### 5.4. *Summary*

In this chapter I have shown and categorised the comments on the nature of science by popular science authors and practising scientists. I have argued that one of the main types of occurrences of comments on science and what distinguishes it, is the quick aside which justifies something as proper science. These types of comment often occur when they are needed to perform that role for some controversial fact or theory, but can also crop up in other places. These comments are slightly different in character from the ones in the more philosophically oriented books, which are able to go more into detail about how the author thinks about science.

I was told almost unanimously that the way scientists learn about the nature of science is mostly informal. It is often left to the student of science to find out him/herself what scientific method is, by either taking voluntary courses, reading books or having discussions in philosophy themselves or more usually simply by doing science and therefore acquiring a technical and *tacit* knowledge of scientific method. There was disagreement on whether this was a good or a bad thing, some scientists thought that learning about the nature of science is not all that important, some thought that those that will become scientists anyway are those that try to inform themselves and think about these matters, while others were strongly of the opinion that more should be taught about the nature of science in a more philosophical rather than a technical way.

The scientists with whom I have spoken gave me a fairly similar spread of opinion about science to those in the books. Mostly science is perceived to work by

constructing hypotheses and then experimentally or observationally testing them, though the details of how that should work vary, and there are other fundamental ways in which science was seen to work, including a more inductive emphasis on hypothesis generation rather than testing, and an emphasis on the social norms of science. These views were rarely seen as mutually exclusive. Finally, I was interested to observe that although academic philosophy of science in France has been different from English philosophy, and although French scientific culture has on occasion been remarked to be more philosophically oriented than English scientific culture, there was not much difference in actual opinions about scientific method.

I also started developing some of the themes that will be visited in the next chapters, the philosophical comment in popular science as boundary work, where the philosophy legitimises controversial science, or shows pseudo-science to be such. Also, the idea that scientists talk slightly differently about philosophy than philosophers do will be shown stronger in some of the other cases, notably in chapter 6 on Occam's razor. Finally, another interesting result, which will also come up at several points again is that there is some slight difference between the opinions in the books and in the interviews, even though it is not very big: mostly there was agreement that science is hypothetico-deductive, but while the interviewees have often talked about inductive ideas, the books have not. The difference between philosophical ideas depicted in the books and in the interviews will be particularly strong in chapter 7 on reductionism.

## Chapter 5: Popper and Kuhn

|                                                                                     |            |
|-------------------------------------------------------------------------------------|------------|
| <b>1. Introduction .....</b>                                                        | <b>129</b> |
| <b>2. Philosophical introduction.....</b>                                           | <b>130</b> |
| 2.1. <i>Popper</i> .....                                                            | 131        |
| 2.2. <i>Kuhn</i> .....                                                              | 133        |
| <b>3. Popper and Kuhn in the popular science books.....</b>                         | <b>135</b> |
| 3.1. <i>Popper: The philosopher as an authority figure</i> .....                    | 136        |
| 3.2. <i>Critical discussions of Popper</i> .....                                    | 140        |
| 3.3. <i>Kuhn: The historian of science legitimising the outsiders?</i> .....        | 142        |
| <b>4. Popper and Kuhn in the interviews .....</b>                                   | <b>145</b> |
| 4.1. <i>The celebrity of Karl Popper</i> .....                                      | 146        |
| 4.2. <i>Falsification and the logic of science</i> .....                            | 149        |
| 4.3. <i>The relative obscurity of Kuhn</i> .....                                    | 157        |
| 4.4. <i>Revolutions, paradigms, and the search for truth</i> .....                  | 159        |
| <b>5. Discussion and summary .....</b>                                              | <b>167</b> |
| 5.1. <i>Popper and Kuhn's ideas in popular science: Philosophers as authorities</i> | 167        |
| 5.2. <i>Reputations predisposing reaction, and philosophers as boundary objects</i> | 172        |
| 5.3. <i>Summary</i> .....                                                           | 174        |

### 1. Introduction

This chapter will present how the popular science books and the scientists interviewed have written and talked about two particular philosophers, Karl Popper and Thomas Kuhn. I chose to concentrate on them for two reasons. First I read the popular science books before talking to the scientists, and these two philosophers get the most attention of all the others by a huge margin, apart from William of Ockham who will be discussed in chapter 6 and who was only ever mentioned together with his famous razor. The other reason is that these two philosophers seem to represent two completely different reactions from scientists, at least during the science wars. While Popper is regularly held up to represent scientific opinions themselves, Kuhn seems to get more negative reactions as a defender of relativism and all that is wrong with philosophical meddling in science. This is of course only a superficial impression gained from the science wars literature and from those scientists who shout the loudest. The aim of this chapter is to find out if that is in fact true.

The cases of the reception of Popper and Kuhn, and of their philosophies, represents an interesting comparison, given the contrasting reputations of these

philosophers. It is also interesting to see how scientists respond to their philosophies who have not in fact heard of these philosophers or what they represent and those that have. The process of evaluating scientists' opinions on Popper and Kuhn cannot easily be divorced from the evaluation of the scientists' own philosophical opinions, and I believe that this chapter shows an interesting array of opinions held by scientists which is not easily represented by the dichotomies of agreeing/disagreeing with Kuhn/Popper, or their philosophies.

This chapter will explore some of the themes introduced in the last chapter, that of scientists using philosophy as boundary work, in particular how the idea of social identity and its boundaries can be applied to scientists' support for Popper: The interpretations of what Popper's philosophy implies and means for the scientists' everyday practice of science are partly shaped by these. I will also introduce the idea of the philosopher as an authority by which that boundary work gets done, and through which the philosopher can become a boundary object in the sense of Star and Griesemer outlined in chapter 3, where the philosopher becomes a "common coin" with which different groups can communicate while still retaining their own interpretations of science. A similar point also applies to Kuhn, where however the expectations on his authority vary – while the interviews show that there was a lot of agreement on Kuhnian ideas, or ideas that can easily be interpreted as Kuhnian, the philosopher himself fulfilled a different role, not so much as an authority (though in some books that happened as well), but as a supporter for more unconventional science. I will therefore introduce the idea that the philosophers' reputations can predispose scientists' interpretations of them.

## **2. Philosophical introduction**

Popper and Kuhn have been very prominent and influential in the general philosophy of science, and their ideas have already been outlined in the introduction to the last chapter, where I have also shown the general contexts in which their philosophies have been formulated. Here I will chiefly introduce the aspects of their philosophy on which have come up most during the interviews.

## 2.1. Popper

Karl Popper is chiefly associated with his philosophy of falsificationism, which he formulated during the heyday of, and in direct response to, the logical positivism of the Vienna Circle. Falsificationism presents Popper's attempts to solve the problem of demarcation and induction, which has been a well known problem in the philosophy of science since Hume. The typical philosopher's way of illustrating the problem is the example of the white swans, which incidentally was often the first thing some interviewees associated with Popper: He is "the guy with the white swans and the black swans".

### *The problem of induction and falsification*

If we observe a large number of swans and all of them turn out to be white, then inductive inference would lead us to conclude that all swans are white. However, there is always the possibility that the next swan we observe is not white, and the swan example is particularly liked by philosophers because it turns out that there are plenty of black swans in Australia. Popper generalised the problem to say that every scientific law has to be of the form "all things that are A must also be B", and the logical point is that no amount of confirming evidence, i.e. instances of A that are B, will ever prove that law. Traditionally attempts to solve the problem involved ideas of trying to find a measure of confirmation, so that we can at least find out when a law has been confirmed well enough. Popper's innovation consists in turning the problem on its head and insisting that instead of trying to prove laws, science in fact tries to disprove them, that is, scientists try to find the one instance of a black swan that proves the law "all swans are white" to be false.

The way science proceeds for Popper is that scientists propose a theory or hypothesis, use it to predict testable observations, and then go and test the predictions. If the hypothesis is *falsified*, then we have to modify it or look for another hypothesis. If it passes the test, then we look for more strict and rigorous tests and proceed to try to falsify it.

### *Theory-ladenness*

The other problem Popper saw with inductive reasoning was that, no matter how much we try, we will never be able to observe anything completely neutrally. We



will always have an underlying theory with which we will interpret the results, and an underlying theoretical framework which directs us where to look in the first place. To illustrate the point Popper tells us how he once started a lecture by telling the students to “observe”, to which they responded by asking *what* they were supposed to observe: “Clearly the instruction ‘Observe!’ is absurd” (Popper 1992 [1963] p.46.).

#### *Falsifiability as a demarcation criterion.*

Popper used his idea of falsification to arrive at a definition of science which is also removed from the logical positivist definition of science. At least the earlier and more dogmatic versions of logical positivism (as described for example by Ayer 2001 [1936]) held that a statement is scientific if, and only if, it is verifiable, and every statement that is not verifiable is not just not science but literally nonsense. Popper used his concept of falsification to a similar end, arguing that a theory or hypothesis is only scientific if it is *falsifiable*, i.e. if we can possibly devise a test which can disprove it. However, in contrast to the demarcation criterion of the positivists, Popper did not argue that non-falsifiable theories are nonsense, they are merely not scientific. Popper famously used his demarcation criterion to argue against psychoanalysis (Popper 1992 [1963], ch.1) by pointing out that whatever the possible evidence that could be found against it, psychoanalysts can (and have) always managed to save their theory, making it in effect unfalsifiable and unassailable.

#### *Underdetermination thesis*

One of the most influential criticisms of Popper’s philosophy of falsificationism which at the same time also argued against traditional logical positivism, was Quine’s famous essay *Two dogmas of empiricism* (Quine 1980 [1953]). Falsificationism may well work in the case of simple examples such as that of the law of white swans which is disproved by the observation of an orange swan. We run into difficulties when we suppose that the simple disproving observation statement is itself dependent on an underlying theory, as suggested even by Popper himself when he argues for the theory-ladenness of any observation (Chalmers 1982 pp. 87ff; Popper 2002 [1934] ch. 5). Therefore the hypothesis is not conclusively

proved wrong because the theories underlying the observation may instead be wrong. In fact, there is possibly any number of underlying theories and background assumptions that could be responsible for a falsification and furthermore it is impossible to say which background assumption is at fault. This difficulty of falsificationism has been acknowledged by Popper himself, as a similar thesis had already been advanced around 1900 by the French philosopher Pierre Duhem (the underdetermination thesis is therefore also known as the “Duhem-Quine” thesis, see Gillies 1993 ch.5 for a discussion of the differences between Duhem and Quine), though it does not seem to have been satisfactorily dealt with by him. This problem has led philosophers after Popper such as Lakatos (1970, 1978) to look for alternatives which try to describe under which conditions a theoretical framework can accumulate falsifications and when it is time to abandon a framework.

## 2.2. Kuhn

In his famous book *The Structure of Scientific Revolutions* (Kuhn 1962), Kuhn argued that rather than being a logical progression of hypotheses and experiments, science progresses by periods of normal science when scientists work within a general theoretical framework (what Kuhn called “paradigm”) which no evidence can call into question, and revolutionary periods where these frameworks are overturned and science eventually settles into a new theoretical framework.

To what extent Kuhn’s philosophy is completely original has been debated, and he himself recounts that he had been influenced by the at the time relatively obscure philosopher Ludwig Fleck (see Hoyningen-Huene 1993 for an introduction to Kuhn’s ideas and his influences). It also echoes in several the development within French philosophy of science by Gaston Bachelard (see chapter 4) of the idea that science progresses by way of periodic “epistemic ruptures”.

### *Paradigms and “normal science”*

Mostly science works within a paradigm, which gives the scientists the problems, methods and interpretations to go about their work. Kuhn did not actually coin the term paradigm himself, which before Kuhn was a relatively obscure grammatical term, though almost every present-day occurrence of the word is derived from Kuhn’s usage. The paradigm gives scientists the methods and the interpretative

framework required for “normal science”, which Kuhn likened to “puzzle-solving”, i.e. working out of problems that are defined by the paradigm. A science for Kuhn is characterised by having only one dominant paradigm, which is all that gets taught in that science’s textbooks and undergraduate courses. Kuhn judges disciplines where there are several competing paradigms as still in a “pre-scientific mode”, the typical example would be sociology where there are several competing theoretical frameworks that make sense of the same data and get taught in the discipline’s basic textbooks.

### *Incommensurability and paradigm shifts*

Kuhn characterises the nature of a paradigm by the analogy of the “duck-rabbit”, a simple picture which depending on how we look at it, can be interpreted either as a rabbit or a duck. The lines on the paper remain the same, while the interpretation of them changes completely. In the same way the typical historical examples of paradigm shifts that Kuhn gives are complete conceptual reinterpretations of the same set of data: In the shift from Ptolemaic to Copernican astronomy, essentially the available data remained the same, as was the case in the chemical revolution where the element of phlogiston was reinterpreted as the absence of oxygen.

The two competing paradigms are so different conceptually that there is no real way of directly translating between them because the same terms have acquired different meanings with the paradigm shift as well, in the same way that the concept of gravity for Newton is not quite the same as the modern-day concept of gravity. The paradigms are essentially made up of different entities and therefore conceptually incompatible. This feature Kuhn called *incommensurability* and it implies that there is no neutral language by which we can translate between paradigms, and that scientists brought up within one paradigm must practically learn the new paradigm similar to the way a new language is acquired. Whether Kuhn was correct in assuming incommensurability between different scientific worldviews, and what exactly characterises incommensurability, is an ongoing problem in philosophical debate (Sankey 1997).

### *Crisis and revolutionary science*

Sometimes the paradigm may be incapable of solving the problems it sets and anomalies accumulate which are set aside at first. There then may come a point when the accumulations of these anomalies becomes so serious that it cannot be overlooked, and scientists start to look for different paradigms to make sense of them. In such a period of crisis, people may come up with competing paradigms, and the science settles into a revolutionary phase until a new paradigm emerges on which the majority can settle and the textbooks get rewritten. In the process of a revolutionary paradigm shift within a science it may also happen that in some respects the old paradigm was better at explaining things.

### *Criticism of Kuhn's philosophy*

Kuhn has received a lot of criticism over his philosophy, notably from some prominent scientists and philosophers who disliked the idea that there may not be a “fact of the matter” that science can discover, and who interpret Kuhn to hold to a caricatured version of relativism which says that whatever dogma scientists currently hold is the truth. The relativist reading of Kuhn has been widespread in the science wars, and is very uncharitable towards Kuhn, who explicitly distanced himself from such an interpretation.

The criticism that sticks however is that it is not totally clear what Kuhn *did* say regarding his attitude towards realism, and he has often been criticised for being very vague not just with his ideas on realism, but also with his concept of paradigm. Masterman (1970) wrote a famous paper arguing that there are 21 distinct uses of the word in Kuhn's book.

### **3. Popper and Kuhn in the popular science books**

In the popular science books Karl Popper was, by some margin, the one philosopher that was referred to the most often, having been mentioned explicitly by eight different authors, out of the sample of 30 books (i.e. 28 different authors). In many other books Popperian themes and ideas on scientific method get mentioned, notably falsification and falsifiability. The second most mentioned philosopher was Thomas Kuhn, who was mentioned five times. Typical Kuhnian terms such as “revolution” and “paradigm” also occurred very frequently.

In this section I will only concentrate on the occurrences of the philosophers themselves, rather than the philosophical ideas that may be considered “falsificationist” or “Kuhnian”, which have to some extent been dealt with in the previous chapter. I have made that decision because actual philosophical convictions are quite hard to identify within most cases – the mere talk of falsifying or of paradigm-shifts, does not necessarily betray any underlying philosophical conviction. As I will show in this section, even when the author has enthusiastically endorsed either philosopher, that did not always mean that they would follow that philosophy. Nevertheless I believe that some interesting patterns emerge: While Popper appears in many short and enthusiastic pithy remarks (see chapter 4) as an undisputed authority on science and even some other areas, Kuhn tends to be discussed more at length, and by a different type of author.

### *3.1. Popper: The philosopher as an authority figure*

Several authors who did not engage in any deeper philosophical debate about scientific method, dropped Popper’s name to reinforce a point they were making, in a similar fashion to the philosophical asides and pithy remarks on the nature of science discussed in chapter 4. The natural historian Richard Fortey, for example, backs up his discussion on ad hoc explanations by remarking that they are

anathema to all those brought up with the scientific and philosophical rigour of Karl Popper and Ernst [*sic*] Nagel. Scientists do not trot out *ad hocs* the way a magician pulls flowers out of a top hat; it is not considered proper behaviour. (Fortey 2000 p.241)

Here there is no explanation of what Popper actually stood for other than “scientific and philosophical rigour”. It is noticeable that in this respect, Popper gets lumped in with another philosopher, Ernest Nagel. These two philosophers would disagree about scientific method in several substantive areas; they do have in common, however that they both in their own ways derive from logical positivism, and their books have become classics in analytical philosophy of science, which traditionally

regards itself as rigorous as compared to continental philosophy<sup>22</sup>. Still, this extract does not in fact show what Fortey's opinions are philosophically. Throughout his book, Fortey describes the scientific community's acceptance of new theories in both Popperian but also social terms that Popper would have vehemently disagreed with. Ernest Nagel's inclusion here is telling towards Fortey's use of the famous philosopher to lend authority to his argument.

A similarly brief appearance of Popper's name occurs in Mark Ridley's "Mendel's Demon" when he discusses the idea that the origin of life was a "fluke":

The argument is interestingly falsifiable, as Karl Popper remarked; it would be refuted if someone managed to resynthesize a living system (Mark Ridley, 2000 p. 23)

Ridley is more observant of falsifiability as a criterion for a scientific claim, thus actually explaining what Popper stood for. But again, Popper's name and ideas only appear to reinforce the author's argument, and get thrown in without much explanation why. In this quote, as in the previous, Popper quite obviously fulfils the role of lending authority to the argument that Ridley is making.

Popper makes an unfortunately much more confused appearance in Stephen Hawking (2001). Hawking quite correctly introduces one of Popper's core ideas, falsificationism. He also quite correctly points out some distinguishing features of logical positivism. He then, quite mistakenly claims Popper to be a logical positivist.

Any sound scientific theory, whether of time or of any other concept, should in my opinion be based on the most workable philosophy of science: the positivist approach put forward by Karl Popper and others. (Hawking, 2001 p. 31)

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<sup>22</sup> Popper himself would have disagreed that his philosophy derived from positivism – during the *positivist dispute* in Germany in the 1960s between Popper, Adorno and their followers over the methods of social science, Popper bitterly complained about being referred to as a positivist by Habermas. This was probably on account of their shared interests in logic and philosophy of science.

Listing the positions that Hawking advocates it appears that he is a committed logical positivist, with a bit of falsification tacked on. This for example is his argument on the nature of time:

If one takes the positivist position, as I do, one cannot say what time actually is. All one can do is describe what has been found to be a very good mathematical model for time and say what predictions it makes. (Hawking 2001 p.31)

In fact, even though Hawking's philosophical position is much clearer and more consistently spelt out than with most other books, it certainly is not quite that of Popper, who was a committed realist. It seems that here again, Popper's name is used to bestow legitimacy onto Hawking's philosophical opinion. Just as Fortey has coupled Popper with the post-positivist philosopher Nagel, Hawking has coupled Popper with logical positivism in general, which Popper would have heavily objected to, see footnote 22 above.

In these examples I am of course not claiming that Hawking or any of the others deliberately misrepresent Popper – more likely Popper has become an iconic figure that represents scientific rigour to some people. Popper, and for some people the positivists (including Nagel), *represent* scientific rigour. In this context, as is the case with the more general philosophical asides discussed in chapter 4, the mere mention that your method follows the philosophy of Karl Popper helps to lend philosophical and even scientific credibility to your claim, this time probably even more so, because Popper is such a widely recognised and scientifically approved philosopher. It is of course unlikely that the author thinks the public readership will be impressed by the dropping of this particular name – let alone that of Nagel – these are after all popular science books. There may however be an element here of the author talking to fellow scientists or even of convincing themselves that their approach is suitably scientific, in the same way that people sometimes delight in

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However, though Popper's philosophy was not positivist, it was established as a direct response to positivism, it certainly shares in the same intellectual tradition (Adorno et al. 1976).

dropping names of obscure philosophers or scientists even (or especially) if they know they will not necessarily be recognised by the audience.

Illustrating that Popper's popularity has spread beyond his philosophy of science, in three of the books Popper gets mentioned for his views on political and historical philosophy and his philosophy of mind, in at least two of these cases the author comments favourably on it, and in both cases nodding approval in a similar manner to the name-dropping fashion of the ones shown above. It is interesting to note in both cases that the actual philosophy of science these authors champion is not necessarily Popperian. While Steve Pinker in both his books from the sample remains mostly uncommitted towards any particular philosopher, Mark Buchanan turns out to be at least at some points positive towards Kuhn's philosophy (see below), which is quite incompatible with (pure) Popperianism. Davies includes a more negative comment on Popper in a discussion on the experiments of Benjamin Libet about the time delay between an act and people's experience of when they make the decision to act. Here, Davies remarks on Popper's (and John Eccles') term "readiness potential", which he describes as a "throwback to Descartes's dualism" (Davies 1995 p. 271).

A possible explanation here is that the author is engaging in the building up of a positive self-concept of their own science by conforming to what they have come to see as a defining group norm: that of adhering to Popper's and the positivists' philosophy of science (see chapter 3 on social identity). Here the author is conceivably not only talking to a lay audience, because a popular science book writer can expect his or her book to be picked up by fellow professionals. But also this social positioning here can be seen as a need to justify to oneself as part of the desirable social group, whether anyone else picks up on it or not.

This interpretation I find more useful than using the just boundary work idea: It is about the author positioning himself within what he finds is a socially desirable group (rigorous scientists), which therefore explains the boundary. The group characteristics that he ascribes to himself are not necessarily those of his actual opinion: this is shown by Hawking's identification with Popperian philosophy even though he has opinions that are quite at odds with Popper. In order to belong to a social group, and to be able to claim so, the author has to display certain group



characteristics, and be visibly seen to do so. With Popper almost personifying good scientific practice, the author will want to be seen to conform to his philosophy. This gives room for a potential conflict if, as it seems for Hawking or Buchanan, Popper's philosophy is actually quite at odds with the author's actual philosophical opinion. This conflict between philosophical identity and philosophical opinion I will argue is particularly obvious in the case of reductionism discussed in chapter 7, and this will therefore be examined in more detail at the end of that chapter.

### *3.2. Critical discussions of Popper*

Two books in the sample discuss Popper's work at some length, and can therefore not easily be seen as merely utilising his good name to make a point as in the manner of the philosophical asides, as both of these books are very philosophical in character. One of the authors, David Deutsch, is a big fan of Popper, and devotes quite a lot of space to defending his ideas. First of all Deutsch acknowledges the huge impact that he thinks Popper has made on scientific thinking. During a discussion on explanations in science he remarks:

Fortunately, *the prevailing theory of scientific knowledge*, which in its modern form is due largely to the philosopher Karl Popper [...] can indeed be regarded as a theory of explanations (Deutsch 1997 p.62, my emphasis)

Deutsch however does realise that there are deficiencies with Popper's argument at places:

The Popperian defence of science as a process of problem-solving and explanation-seeking is not sufficient in itself. (Deutsch 1997 p.76)

This comment is then followed by a further critical development of Popper's philosophy by Deutsch. Curiously Deutsch's criticism of Popper's philosophy is defended by appealing to the rational nature of science, which would in turn surely appeal to Popper himself. In one chapter he sets up a fictional debate between a "crypto-inductivist" and himself about the validity of Popper's philosophy, and inductivism in general.

CRYPTO-INDUCTIVIST: [...] You make a careful distinction between theories being justified by observations (as inductivists think) and being justified by argument. But Popper made no such distinction. And in regard to the problem of induction, he actually said that although future predictions of a theory cannot be justified, we should act as though they were!

DAVID: I don't think he said that, exactly. If he did, he didn't really mean it.

CRYPTO-INDUCTIVIST: *What?*

DAVID: Or if he did mean it, he was mistaken. Why are you so upset? It is perfectly possible for a person to discover a new theory (in this case Popperian epistemology) but nevertheless to continue to hold beliefs that contradict it. The more profound the theory is, the more likely this is to happen.

CRYPTO-INDUCTIVIST: Are you claiming to understand Popper's theory better than he did himself?

DAVID: I neither know nor care. The reverence that philosophers show for the historical sources of ideas is very perverse, you know. In science we do not consider the discoverer of a theory to have any special insight into it. On the contrary, we hardly ever consult original sources. They invariably become obsolete (Deutsch 1997 pp.156-157, original emphasis)

This enthusiastic defence of Popper even when Deutsch at times concedes that there are deficiencies to his philosophy is in itself quite telling towards the iconic status that Popper seems to hold. It is of course also quite ironic, given Deutsch's irritation about the historical reverence shown to philosophers, because as a philosophical commentator, Deutsch has other options available when confronting shortcomings of Popper's work: philosophically speaking there is not that much difference between claiming Popper was wrong because of problem A, and claiming that Popper was right, apart from when he said A. Here there is a problem of positioning, and exactly the same problem has in fact been faced by philosophers critical of Popper as well. One famous example is Lakatos who at the beginning thought of himself as adding to Popper's falsificationism (he called his

approach “sophisticated falsificationism”), but whose philosophy is now considered important in its own right (see Lakatos 1970).

So here again, even though Deutsch’s work is in fact a critique of Popper’s philosophy, Deutsch feels the need not to distance himself too much from this authority. This I think is entirely compatible with Popper’s philosophy being a social group characteristic as described above. Furthermore, Deutsch shows that it is possible even to consciously critique Popper’s philosophy and still identify with it.

The sole author in the sample who spoke out against Popper was Ernst Mayr. Mayr has written a book that is very philosophically orientated, and in which he explicitly confronts traditional philosophy of science. However, even in this context, Popper at least gets away better than any other (normative) philosopher:

I do not know of a single biologist whose theorizing was much affected by the norms proposed by philosophers of science. Scientists usually go about their research without paying much attention to the fine points of methodology. The one exception is Karl Popper's insistence on falsification (see below), which was widely accepted by biologists in principle, though it rarely worked out in practice. (Mayr 1997 pp.46-47)

Regarding his comments on his scientific colleagues’ attitude towards Popper, in fact Mayr’s point is not too far removed from the argument of Mulkay and Gilbert (1981) in their study of scientists’ talk of Popper (see chapter 2). Mayr argues that many people merely claimed to follow Popper:

When in the 1950s and 60s Popper was the great rage, every biologist I knew insisted that he was a Popperian, and then did whatever he wanted to do. Labels are sometimes politically convenient but they often mean nothing (Mayr 1997 p.55)

### *3.3. Kuhn: The historian of science legitimising the outsiders?*

The next most discussed philosopher was Thomas Kuhn, who was mentioned by a total of five authors. There are some similarities in the type of attention Kuhn gets,

but also some striking differences. In general Kuhn, when he does get mentioned, receives a lot more attention, and his ideas are explained and dwelled upon. However, like Popper, Kuhn still gets drawn upon when the author wishes to back up a particular point with some philosophical authority. Thus in a chapter titled “Hindsight”, the computer scientist John Naughton writes about

Thomas Kuhn, the great historian of science whose book *The Structure of Scientific Revolutions* radically altered the way we think about intellectual progress (Naughton 1999 p.110)

Kuhn’s philosophy effectively gets a whole chapter to itself, the author trying to explain (with hindsight) why some people resisted a technological invention which turned out to be quite useful.

Another computer scientist, Steve Grand, is also very positive about Kuhn, ending a particular discussion with a nod to him:

In short, I believe we are undergoing what Thomas Kuhn called a paradigm shift. (Grand 2003 p.23)

In fact, his championing of Kuhn is neatly illustrated by how obvious Kuhn’s philosophy really is for him:

In many ways, philosophy is the art of stating the obvious, but until someone actually stands up and says so, most of us continue to leave our assumptions unquestioned. (Grand 2003 p.23)

In both these cases a considerable amount of space is devoted to discussing Kuhn, or ideas that are later identified as Kuhnian. Although this is different to the way Popper was mostly discussed, there is still, in both cases something of an acceptance of Kuhn’s ideas, and they are represented as more or less uncontroversial. In the case of Grand’s book that is even more visibly the case as he is describing Kuhn’s ideas as being obvious.

Mark Buchanan's comments are along the same lines, though his commentary on Kuhn is much shorter. This however has to be seen within the context of Buchanan's equally admiring reporting on Popper's (political) philosophy (mentioned above).

For Thomas Kuhn, an influential historian of science, the essential distinction between revolutionary science and ordinary science is that the former involves a "tradition-shattering" as opposed to a "tradition-preserving" kind of change (Buchanan 2002 p.42)

One aspect where these descriptions differ from the discussions of Popper is that Kuhn gets called a "historian" by two authors, rather than a philosopher. This may reflect the feeling that a historian will be more empirically trustworthy than a philosopher. After all, instead of offering an opinion, the historian ostensibly reports facts about what happened. Quoting Kuhn as a historian rather than a philosopher may therefore make his conclusions more compelling. That is reflected in a way even in the comment by Grand who *does* call him a philosopher, but nonetheless needs to argue that Kuhn's point was obvious and therefore in a way an objective one. There may however not be too much merit in this interpretation, as Kuhn is often seen as a historian rather than a philosopher even outside the popular science context. It is clear in any case that, even though Kuhn appears more often than any other philosopher after Popper, these three authors felt they needed to explain Kuhn's ideas in more detail. And yet, Kuhn the historian or the philosopher is still clearly described as an authority figure.

The two books that are sceptical of Kuhn's philosophy are the two philosophical books that also treated Popper with some scepticism (Deutsch and Mayr). As a supporter of Popper's philosophy, Deutsch unsurprisingly finds that he cannot support Kuhn. Deutsch's main objection towards Kuhn is the overemphasis of social contingencies over the logical structure of science, which in fact echoes the whole debate about naturalistic philosophy versus normative philosophy.

Kuhn's theory suffers from a fatal flaw. It explains the succession from one paradigm to another in sociological or psychological terms, rather than as having primarily to do with the objective merit of the rival explanations. Yet unless one understands science as a quest for explanations, the fact that it does find successive explanations, real objectively better than the last, is inexplicable. (Deutsch 1997 pp.323-324)

The other critic of Kuhn, Ernst Mayr, complains just like he has complained about most other philosophers too, that Kuhn's philosophy only really applies to physics, rather than biology or science in general.

Virtually all authors who have attempted to apply Kuhn's thesis to theory change in biology have found that it is not applicable in this field. (Mayr 1997 pp.96-97)

In this remark he basically repeats what he disliked about normative philosophers of science generally (see chapter 4): they really only philosophised about the physical sciences.

#### **4. Popper and Kuhn in the interviews**

In the interviews, the names of Karl Popper and Thomas Kuhn (and even some other philosophers) would sometimes fall spontaneously into the initial conversation on what is science (see chapter 4), though not always positively. Otherwise, when I came to the question of what people thought about Popper's or Kuhn's philosophy, I would ask whether the respondents were familiar with the names, and their philosophies.

This section presents the results in two subsections each for the two philosophers. The first describes people's reaction towards the names of the philosophers themselves, and the reactions towards the philosophies of people who are familiar with them. Then the second section presents the responses towards the philosophical ideas themselves, falsificationism in the case of Popper and the idea that science progresses through paradigm shifts in the case of Kuhn. Thus the first part is essentially about the interviewees' reactions to the philosophers and the

second part is about their reaction to their philosophies. I have chosen to do this firstly to make the comparison with the popular science books easier, where I have focused more on the reactions towards the philosophers. But also I think the level of recognition these two philosophers receive, and how that translates into an acceptance or rejection of their philosophies, is fascinating in itself, and it may be worthwhile to compare those responses to those of people who only learned of these philosophies through my own introduction, or to the informal education on methodological issues that they received themselves.

#### *4.1. The celebrity of Karl Popper*

As was the case in the popular science books, Karl Popper turned out to be a very well recognised name. When introducing the topic I generally asked whether they were familiar with the name, and 23 out of the 40 interviewees responded positively. A further 6 even spontaneously mentioned Popper without any prompting from me.

These figures hide a wide variation of reactions towards the name. As shown, a considerable number knew enough of Popper to discuss his ideas with me spontaneously, but not all of them were entirely favourable towards Popper. There were also a number of respondents who replied that they knew the name, but had no idea what he stood for, others were reluctant to comment on his philosophy, saying that they would probably get it wrong.

Among those who were familiar with the name were unsurprisingly the group of scientists who had at some point in their education taken philosophy and/or history of science courses, though one scientist who said that he took a module discussing some of the methodological and ethical issues underlying science as part of his PhD studies in Canada (13 Mid Career, Biology, Male, UK) did not recognise the name Popper. Another small group of scientists had tried to educate themselves in philosophy of science and invariably had heard of Popper, even if they had not gone out to read him themselves. Finally, a large group of scientists were just familiar with the name, but did not or could not say where they heard the name from and these people were also most unsure about what philosophy he actually stood for, suggesting that the name of Karl Popper has penetrated at least into some scientists' general knowledge.

The level of recognition afforded to Popper is in a way unsurprising (see Mulkey and Gilbert 1981 discussed in chapter 2). However, while Popper was indeed very well recognised, that alone did not mean that his philosophy found much favour. Some scientists had specifically built their whole understanding of science around Popper, though that was a fairly rare reaction. One scientist actually decided that Popper had in fact described science very well, after having heard negative opinions about Popper in his own philosophy education:

[In the philosophy course] Popper was a dirty word [laughs]. Well, that's how I remember it, I mean... I think the general idea was that science is more complicated than that. And that a lot of it is social influences and so on and so forth. But now I've been practising science quite a lot longer than I had then, [...] you know, clearly falsification is absolutely key, if [*inaudible*] going to have to test hypotheses, then falsificationism is what a lot of science is about. (12 Senior, Biology, Male, UK)

Very negative reactions towards Popper however also occurred often. During a discussion of science in general during one of the pilot interviews, this scientist showed a lot of dissatisfaction with Popper:

I think Popper's rejection of induction as a means of developing scientific theories and models is just crass in the extreme. (1 Senior, Physics, Male, UK)

Mostly, though, the reactions to Popper were ambivalent. As I will show below when I discuss the reactions to falsificationism, most of those who had heard of Popper and had mulled over the philosophy of falsificationism applied to their own day to day scientific life, decided that there has to be more to science than just falsificationism, and that things like verification and induction have to have a role as well in any philosophy.

There can be a lot of similarity between agreeing with a philosophy and therefore in a way identifying with it, and on the other hand rejecting it while



holding a fairly similar opinion. This is often manifested by followers of Popper still ascribing a large role to verification as well as falsification in science.

Scientific method is that within some system you make hypotheses, you make testable hypotheses, you test them, and you keep the ones that are verified, and you throw out the ones that are falsified. (32 Senior, Physics, Male, France/En)

This scientist had not taken any particular courses in philosophy, but was very aware of Popper, admitting though that he does not know much of his work. Asked whether he accepted falsificationism as a philosophy, he replied, laughing: “Nothing else to say. I buy that one!”. This shows that even when Popper has clearly influenced a scientist’s thinking towards scientific method, his precise teachings are not necessarily taken over wholesale. In this case it is, as the scientist admits himself, because he does not know that much about Popper’s philosophy, and has only taken on board those aspects of it that he heard being discussed informally, and those he agrees with.

The role he ascribes simultaneously to verification and Popper’s philosophical authority is rather fascinating. Having explained a particular case from his own experience where he thinks to have verified something, he goes on to imagine Popper’s response:

So I think for me, from a more or less a logic layperson’s point of view, we verified the hypothesis [...]. I guess Karl Popper is going to tell me that it could be many other things because we haven’t tested every possibility [laughs]. (32 Senior, Physics, Male, France/En)

While the logic of science according to Popper may dictate rejecting verification, actual science manages quite well with it. Through this intervention of the imaginary Popper, this scientist manages both to defend the way he does his science, and yet defer to the philosophical authority of Popper, to which he interestingly, but mildly sarcastically, subjects his own scientific work. Regarding philosophy of science and logic, he regards himself a layperson.

It is also possible to explicitly disagree with Popper, because there is more to science than falsification. Another scientist who also has heard of Popper through informal discussions and self-study, but admits to not having read his work carefully, reacted ambivalently when asked if he agrees with Popper, making it his first point to stress that there is more to science than falsification.

I think you can have parts of the scientific process which are valuable which are not hypotheses. In other words, I think you can carry out things like thought experiments and come up with ideas which are valuable aspects of the scientific process, but are not themselves falsifiable. And this is also... Something which I think is underrated now is the importance of technology in science. (6 Senior, Chemistry, Male, UK)

This criticism is similar to the one that respondent 1 had above about Popper neglecting the aspect of science surrounding theory formation. Respondent 32 imagines that Popper has an answer to his argument against strict falsificationism, and seems happy to submit to Popper's better philosophical judgment concerning that argument. Respondent 6 by contrast appears more skeptical towards Popper in the sense that he was prepared to give Popper the benefit of the doubt, (as he qualifies his remarks by admitting to not having read Popper extensively), but nevertheless criticizes falsificationism from the outset. By contrast, respondent 1, who has a similar reason than 6 to criticize Popper, rejects him outright.

#### *4.2. Falsification and the logic of science*

After asking the scientists specifically if they have heard of Popper, I proceeded by explaining the general principles of falsificationism as best as possible. There were some people who were familiar with the term falsificationism or falsification who were not familiar with the name Popper, just as there were also people who have heard of Popper but were not familiar with the word falsificationism, or even the concept of falsificationism. One interesting fact was that, although I do not think there was much difference between the level of recognition of Popper himself between the UK and the France interviews, the word "falsification" was generally not recognised in France unless the scientist was particularly interested in

philosophy<sup>23</sup>. One obvious explanation would be that Popper's word for falsification was translated differently in his French translations, this however is not the case, as the French translation of the *Logic of Scientific Discovery* also talks about "falsification". (However, in Andler et al. 2002, a French textbook on the philosophy of science, Popper's falsificationism is referred to as "faillibilisme popperien", p.181.)

I generally explained falsificationism through the famous philosopher's example of the white swans or the black ravens (see section 2 in this chapter). I especially stressed, as was important for Popper, that according to falsificationism there is never any conclusive proof of a law, theory or hypothesis in science, but that we can be sure when we have disproved or falsified it. I explained that Popper saw science as progressing chiefly as the process of hypothesis construction and the search to refute them. I also explained that this very brief introduction is hardly doing Popper much justice, as he developed a much more elaborate version of his philosophy, but nevertheless it captures the most important points. I then asked if that was a reasonably accurate portrait of how science works, and if they answered positively, I asked if they pursue such a methodology deliberately or whether they think that science works like that unconsciously.

### *Initial reactions*

Initial reactions to the concept were fairly evenly split. There were 13 cases where I was happy to label the reaction as positive, 10 which I classed as negative, and in the remainder I found some acceptance of the concept but also a lot of skepticism. I have shown some of the reactions of some of the scientists above who have heard of Popper and in some cases even read him. One or two of the scientists who were not familiar with Popper thought that my version of falsificationism is a very good description of how science works, maybe arguing that there is possibly more to science, but essentially agreeing:

Yea, I think... I mean most of the [inaudible] is disproving it. I think once you have done that you have to make new theories, because a particular

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<sup>23</sup> A small number of respondents, both in France and the UK, understood "falsification" to mean the deliberate faking of experimental results.

thing is so... I think it's basically a cycle, so probably what you said was just one half of the cycle, so then you make new theories or something like that, and probably somebody comes along and disproves your theory. (22 Early Career, Physics, Female, UK)

Apart from this scientist however, who does nevertheless identify that there may be more to science than just falsificationism, most scientists who reacted unquestioningly positively towards falsificationism were familiar both with Popper and his philosophy.

A number of the interviewees have felt strongly enough about falsification that they even included it in their initial description of scientific method. This for example was written in an email response by a scientist who was not able to be interviewed, but nevertheless kindly wrote me a brief description of how he sees scientific method:

As scientists we generally understand that science progresses when experimental data becomes sufficiently accurate that it can refute hypothesis. Any hypothesis that withstands extended tests of disproof eventually becomes a theory – but remains as accepted theory only so long as new experimental measurements are unable to show up its faults. (41 Mid Career, Physics, Male, UK)

Similarly, when asked to contrast science with something that is mostly agreed on not to be science, some answers specifically emphasised that science can be falsified. For example I asked an astronomer why astrology is not science; this person later told me that he had tried to read some of Popper's work (he found it rather dry):

If you want, every physical experiment, every physics experiment in thermodynamics in a sense is testing [fundamental thermodynamical] principles. And we take them as principles as long as they're not proven wrong. Who knows, maybe one day they will. I doubt it but maybe it will happen. (16 Mid Career, Physics, Male, UK/Fr)

*Falsification is “not the whole story”*

Probing deeper into the initial reactions, one complaint that I have heard very often is that surely there must be more to science than just falsification. The following view is from someone who was very interested in philosophy, to the extent that he even replied to my initial approach by saying that he may not be “philosophically naive enough” (36 Senior, Biology, Male, France). He agreed that a lot of science works through falsification, but that Popper did not give enough regard to the accumulation of data which forms an integral and important part of science. Similar opinions have in fact been voiced fairly often. Respondent 1 who I quoted above for example saw Popper’s apparent lack of interest in how theories get formulated in the first place as “crass to the extreme”, and while this may be a particularly strong expression, there has been a general unease about the emphasis placed by falsificationism, and sometimes philosophy in general, on the justifying the hypotheses rather than coming up with them in the first place (this has been called the “context of justification”, as opposed to the “context of discovery”):

[falsification] does come into play, but it’s not my thinking about science. My thinking about science is to see what’s happening. And then try to accumulate evidence about whether we can form an hypothesis. So, so, in fact it’s more... I don’t know, it’s less hypothesis-driven. (20 Mid Career, Biology, Male, UK)

A different but similar complaint was that falsificationism ignores the considerable impact of confirmation or even verification. While every scientist agreed that we can never prove a general law, a lot of them nevertheless thought that these concepts have at least some role to play in science. In this they do not actually deviate too much from Popper’s opinion, as even Popper accepted the point, and included a chapter on “corroboration” in his famous *Logic of Scientific Discovery* (Popper 2002 [1934] chapter 10, introducing even a quantitative measure of corroboration later in appendix \*ix). Popper however gave corroboration a much less distinctive role in the logic of science than it usually assumed in the more induction oriented philosophies, especially in the positivist philosophy that Popper

criticised in his book, which is reflected by his refusal to call that concept “confirmation”. In a discussion of the value of confirmations and proofs in science in the light of the fact that we can never completely prove a law, interviewees often pointed out that at least we can get to a degree of certainty that for all intents and purposes is as good as a complete proof.

Yea, I mean... so experience tells us that Newton’s laws of motion work pretty much all of the time, I mean Einstein proved that it wasn’t always true, but you know, it’s pretty much good enough to get a man on the moon, and so it’s something to... you might as well live your life assuming that it’s true, because to a very good approximation it is, and that’s why I would call it a law. (10 Early Career, Physics/Chemistry, Male, UK)

A less strongly worded opinion on this point was that Popper’s philosophy did not reflect that scientists in fact prefer to cling on to theories rather than falsify them, even if falsificationism is in principle right. This in particular came from an interviewee who on the whole was very positive towards Popper, which probably shows that even if a scientist accepts a lot of what Popper says about science, it does not mean that he/she thinks that science always works or even should work like that. In any case, even though this person was one of the more enthusiastic about Popper in general, he also had misgivings about being called “Popperian” (see also his earlier comment on the education he received where Popper was a “dirty word”)

[falsification] is not the whole story. I mean I certainly don’t think it’s the whole story, but if hypotheses... it’s key to hypothesis testing, your ideal is to be able to falsify one hypothesis, and hopefully have something else that fits to that better in its place. But that’s not the whole story, you become attached to hypotheses and don’t actually want to falsify certain hypotheses (12 Senior, Biology, Male, UK)

### *Underdetermination thesis*

Other criticisms of Popper that came up are various forms of the underdetermination thesis. Underdetermination did not only come up during discussions of Popper, either. For this scientist it was a more general problem faced by science, and any variation of a dogmatic hypothetico-deductive approach.

That somehow seems an odd way, doesn't it, that you've got this hypothesis, and no matter what the world throws at you, data wise, in some sense, you can always rewrite all your theory, but your aim is underlying, is always being to keep that hypothesis as true (21 Mid Career, Physics, Male, UK)

In this sense underdetermination also links to one of the other themes that came up during the interviews (see below on the results regarding Kuhn), which is that of people being biased towards particular theories, usually their own, and will therefore blame other factors such as incompetent postdocs or other contingencies for a falsification. One scientist even had the idea (admittedly half joking), of introducing blind testing into physics or biology to eliminate that expectation bias (18 Mid Career, Biology/Physics, Male, UK).

But it is not just the human fallibility in supporting one's own theories or in misreading the results which gives rise to underdetermination. Some people have remarked upon there being an inherent uncertainty about the data which means that you can never really falsify anything. The most obvious case concerns statistical evidence, and so it is no surprise that this was brought up by a medical biologist:

But then you get into statistics, and the inherent variability in all systems means that at any time you have to say, right, ok, I've disproved this, and my certainty of that fact is ninety five percent, or whatever it is. So actually there are still questions that are raised usually whenever you make a particular claim, advance, whatever you call it, fact... and somebody else may claim to have disproved what you have just proved, or vice versa. So, I think it's all tending in the same direction eventually. (8 Senior, Biology, Female, UK)

There was, however, certainly among the people who were positive towards Popper, a feeling that the underdetermination thesis was a little bit unfair towards falsificationism. If a theory has been falsified, then of course we try to rescue it by modifying it, or background assumptions. “Did Popper say you’re not allowed to refine your hypothesis? Surely not, I mean... [laughter].” (12 Senior, Biology, Male, UK). So in these senses the underdetermination thesis is not so much a problem for science as part and parcel of it. It seems that in fact a lot of the actual opinions that many of Popper’s sympathisers among my interviewees in fact held to an opinion of science which is more sophisticated than the pure “dogmatic” falsificationism and in fact combines falsificationism with the underdetermination thesis and even ideas on revolutions and paradigms that I found very reminiscent of the philosophies of Lakatos (1970) or Laudan (1977).

As with the quote I presented earlier by respondent 32 that Popper would probably show him wrong, there is some kind of expectation of Popper that probably is best explained by his reputation for being such a famous and, among scientists, well accepted philosopher. Compare this with the effect observed in the popular science books, where the name of Popper is attached to some quite different philosophies which are in fact closer to the author’s actual opinion of science, especially Stephen Hawking’s identification of Popper’s philosophy with positivism. There may be some kind of deference towards Popper (at least when it comes to more philosophical issues) that seems to shine through in Hawking, and the comment by respondent 32, what is more apparent in many of Popper’s supporters is that when Popper’s philosophy seems to contradict their natural expectations and experiences of science, there is a reaction that he surely cannot have meant it like that. In this way there is still the expectation that Popper can be saved by appeal to common experience as a scientist. And this also seems to echo the way criticism to Popper is treated by David Deutsch shown earlier in this chapter: Popper did not mean it like that, or if he did he would surely change his mind now that there is a good counterexample.

#### *Social factors influencing falsificationism*

A final point that came up with discussions about falsificationism was one that related more to the social norms in science, and how they either impede or unjustly



favour adherence to falsificationism. A social factor favouring falsificationism that was picked up by some people was that Popper's philosophy of falsificationism has become so pervasive especially among the funding agencies, that science which does not conform to that way of thinking about science stands little chance of getting funded. This point applies in fact not only to Popper, but in general to hypothetico-deductive ways of conceptualising science as "theory testing".

Actually there's one thing about [the] experimental approach, which I think is still valid within a scientific context, and it's actually very important within [the] current scientific climate. That is to a certain extent very descriptive science, you know, I've focused on testable hypotheses, and that's actually fundamental to getting our grants funded, if you don't have a clearly formulated hypothesis it's difficult to get your work funded. (13 Mid Career, Biology, Male, UK)

Balmer (1994, see chapter 2) has also described that phenomenon during his study of the decisions that lead to the rejection of funding for Australia's genome project, which did not involve any identifiable hypothesis, but merely consisted of trying to observe things.

Conversely, there is a social factor that *inhibits* adherence to falsificationism, which interviewees pointed out to me several times: It is very hard to publish negative results, and especially with the publication pressure that academics are under at the moment (in the UK as much as in France), publishing negative results against received wisdom is increasingly seen as a risk not worth taking. One respondent talked from personal experience, which deserves a slightly longer quotation:

I think I am not searching for something wrong, I am searching for something right, yes, because I think both are important, for me, but the problem is that in the scientific world the negative results, so if you have, if you are sure that it's not the way it is, you can't do anything about that, I mean you can't publish papers. And our career is based only on papers, and all that, for us it's really important, and in France more than everywhere else,

so... we need papers, and you can't publish negative results. I know someone that has a really good PhD that has proved that... the hypothesis is wrong and it's not working this way, but he couldn't find a real way, because he didn't have enough time, because three years, and so... but he has proved that this is not the way, [...] it was obvious. And he couldn't have a paper with that, so that's why we need to find right hypotheses. I think it's stupid, because the problem is that if you have found that something is wrong, if it's not published, then your neighbour will do the same thing two years after you, and then we are losing a lot of time and a lot of money because of that. (26 Early Career, Biology, Female, UK/Fr)

#### *4.3. The relative obscurity of Kuhn*

By contrast to Popper, Thomas Kuhn was not very well recognised, having spontaneously been mentioned by about six people (most of which are the same who spontaneously brought up Popper, and belong to a group of people who were generally very interested in philosophical matters). Ten further people said they have heard of the name. Rather than being a philosophical doctrine that stands out as a rival to that of Popper, some scientists have remarked that they are very happy to believe in both falsificationism and Kuhn's philosophy. In fact, Kuhn's building up of anomalies has been interpreted as an accumulation of falsifications (see the last subsection of 4.4 below).

Next to qualified support of Kuhn I also often got the argument that, though revolutions do happen, they are not really happening that much in biology. A similar argument was made by several biologists, including one who seemed to be otherwise quite sympathetic towards Kuhn's philosophy, who initially reacted positively towards Kuhn (see discussion below on revolutions and paradigms).

On the other hand Kuhn, like Popper, made occasionally negative impressions as well, very clearly expressed by this astronomer who not only heard about Kuhn, but has also read him. Though he admits that he may be somewhat uncharitable towards him, he still has very strong feelings about Kuhn's philosophy:

it's been a year or so since I read [The Structure of Scientific Revolutions], but it seemed to me to be a combination of the blindingly obvious together with completely unprovable and yet seemingly profound statements that you [*inaudible*]. This whole concept of a paradigm shift, either I could read it in a sense in which it told me absolutely nothing that anyone who's been anywhere near science knew, or it was complete rubbish. (2 Early Career, Physics, Male, UK)

This however was the strongest reaction towards Kuhn that I have heard. More prevalent was a more guarded attitude, saying for example that there may be something to his philosophy, but that there is more to science than revolutions. That itself is of course completely compatible with Kuhn, for whom “normal science” was an integral part of scientific progress, however, for many interviewees, that part of Kuhn's philosophy was not the most visible – the concepts from Kuhn that stay in people's minds are more likely revolution and paradigm shifts rather than the more boring normal science. However, there may be an element of me having explained Kuhnian philosophy rather badly, and I will therefore not read too much into that. There were however a couple of people familiar enough with Kuhn's philosophy for me not to have to explain it, who reacted similarly.

#### *An aside on Bachelard and French epistemology*

A French scientist (37 Senior, Biology, Male, France) who reacted fairly positively towards Kuhn was in fact a follower of the French philosopher Gaston Bachelard whose philosophy of “epistemic ruptures” has been compared to Kuhn's philosophy (see my introductions to Kuhn's philosophy and to French philosophy, chapter 4). In fact, when talking about revolutions, this scientist used the Bachelardian term of “rupture”<sup>24</sup>. He was conscious of the similarity between Kuhn and Bachelard, and made that connection clear. After that (fortunately this was the first interview I held in France), I asked most of the interviewees in France if they had heard of Bachelard (see chapter 4 for a more detailed discussion of the differences between the UK and France). However, I found that even though the name was almost

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<sup>24</sup> The interview was held in French.

universally recognized there, he was not recognised as a philosopher of science. In fact, even though Bachelard is one of the towering figures in French epistemology, he is mostly known for his other philosophical work, such as his philosophy of poetry. Two of the more philosophically active French scientists that I interviewed were aware of Bachelard's philosophy of science, but did not connect it to Kuhnian terms of revolutions and paradigms in the manner of respondent 37.

#### *4.4. Revolutions, paradigms, and the search for truth*

While many people at least recognized the name of Thomas Kuhn, there was much less recognition of which philosophy he actually stood for. My explanation at this point of Kuhn's philosophy was hampered considerably by the fact that, unlike falsificationism and Popper, the term revolution in science has a widespread acceptance that does not necessarily reflect recognition of Kuhn's philosophy. Even the word "paradigm", which at least in this specific context was first used by Kuhn has taken on a life apart from Kuhn's philosophy that while it was often recognized, it was rarely brought into context of Kuhn. One scientist (who was familiar with Kuhn, but by name only) even was surprised to hear that the use of the word "paradigm" can be traced to one single philosopher (19 Mid Career, Chemistry, Male, UK). The word "paradigm" seems to have acquired a negative connotation:

[The word "paradigm"] ruined our life, I have to say, I never use that word. [...] It seems to be constructed to specifically to deliver a piece of information, but actually it doesn't reach me, whatever that piece of information is, so that's not one of my normal terms. Because you do hear a lot of people say 'oh this paradigm, so and so, so and so...', what are you talking about? English would do fine, you know? (8 Senior, Biology, Female, UK)

I am in a way less confident than in the case of Popper that in the interviews I have explained Kuhn's philosophy well enough to do him complete justice. In my effort to emphasise the novel aspects of Kuhn's philosophy with regard to Popper's falsificationism and the hypothetico-deductivism that many interviewees saw as the essence of scientific method, I probably overemphasised the revolutionary aspects

over the normal science that for Kuhn plays as much a constructive role in science as the revolutionary periods. Therefore, I believe that many of the negative initial responses that I received towards Kuhn's philosophy are more my fault through insufficient emphasis on the value of normal science. Several scientists have told me after my question of whether science advances by revolutions that they think that it does very often, but that this is not the whole picture either, and in this they are not as much in disagreement with Kuhn's philosophy as my introduction to it may have seemed to them. In part I believe my problem lies in the fact that Popper's philosophy is very easily summarised and very easily put as a simple slogan for scientists to follow, whereas Kuhn's philosophy is much more nuanced. In fact the apparent simplicity of Popper's philosophy may be one of the factors that contributed to much of his success .

On the other hand my explanations may often have been too convincing as well. As one scientist told me (38 Senior, Physics, Male, France), the way I described the seemingly contradictory philosophies of Kuhn and Popper made him agree with both. If I wanted to arrive at his real feelings towards them, I should give him some time to mull it over. In this respect the reaction that the scientist feels he has to think about it more carefully before committing an answer is certainly very interesting. But at least for the case of Kuhn, where the issue is not as easily described as falsificationism, simplicity or even reductionism, the way I present the case, how persuasive or unpersuasive I was during the interview influenced the answer people gave (see chapter 1). Therefore I would be more happy to see this section as a discussion over the issues of revolutions, paradigms and truth in science (and what people understood by those words) rather than about a coherent philosophy as such, unless that was the stated understanding of the people familiar enough with Kuhn's work.

### *Initial reactions*

With the caveat aside that I may not have always described Kuhn's philosophy to its best advantage, it is perhaps surprising that it still received a mostly positive reaction, where 18 reacted quite positively and only 5 were overtly negative. There are very many different interpretations of what revolutions and paradigms mean for science, and I have therefore presented the initial reactions I received towards my

explanations of these terms according to the various interpretations they received and what their role is in science.

I presented my question usually by contrasting Kuhn's philosophy to the logical approach favoured by the hypothetico-deductive model, and that it emphasised that in real science scientists generally work within a worldview which is not easily refuted both because of more social factors as well as the underdetermination principle that we can always find a way of saving a falsified theory (or conversely, rubbishing a well confirmed one). This is a point where many of my interviewees found that this is a much more accurate version of how science actually works than the logical hypothetico-deductive version of science that they themselves gave me earlier. For example, the futility of an individual's struggle against a paradigm was pointed out more than once. One interviewee complains about how unfairly real science can sometimes function with an example of a friend of his, whose ideas were not accepted, though he turned out to be right after all. At the end

on his own [he] couldn't attack this paradigm because he needed more critical mass, so the thing that upset me about the whole process was the extent to which the people who'd been resisting change in fact then became the embracers of it, you know, it didn't seem right in a way. (13 Mid Career, Biology, Male, UK)

Here there is a sort of acceptance of paradigms as a feature of real science, and how hard they are to overturn, which does not mean to say that it is a good thing, in fact they are more an obstacle produced by human fallibility to be overcome.

The crucial concept of paradigm shifts/revolutions and the contention that science progresses through such periods of complete shifts of worldview was regarded as more contentious by the interviewees, but nevertheless found a some sympathy. Again, there are different ways of taking my question of whether they thought that science progresses through paradigm shifts. Some people unequivocally said that yes, they think so, while others agreed in principle, but

argued that “true” revolutions are so very rare that it is not worth thinking about much.

Others have even argued, echoing Mayr’s general complaint about philosophers that Kuhn was probably right for the physical sciences, but not really for the biological ones (or even chemistry in one case), which are more hands on, and where the evidence is less dependent on an underlying theory. This is a view of somebody actually very familiar with and also otherwise sympathetic to Kuhn’s work:

Most biologists do not consider that you have really paradigms in biology. Maybe you’ll remember there was a kind of dialogue between, when Kuhn was dead, in journal like BioEssays, whether the model of Kuhn was susceptible to be applied to biological sciences as well, I think many biologists consider that it’s not so easy to speak about paradigm in biology. For instance molecular biology, maybe it was a paradigm, but it’s impossible to say what was the previous paradigm [...] So I think, nevertheless, the expression “change of paradigm” still has an important place. But only very vague meaning, dramatic change of model. (36 Senior, Biology, Male, France)

Contrasting with the opinion that revolutions are rare, and in biology especially, there were also people who agreed that paradigm shifts happen very often, in biology as much as anywhere else.

Took this guy Kuhn to actually think about it and write it down? Good. Yes, I think it happens all the time, it happens very, very slowly, but it’s something that I... thought about a while ago... (25 Early Career, Biology, Female, UK)

Finally there were also some very negative assessments of revolutions and paradigms. One very strongly worded remark by respondent 2 who has actually read Kuhn’s book *Structure of Scientific Revolutions* was quoted above (in section 4.4), but in general I found that negative evaluations to the idea of paradigm shifts

and revolutions were qualified by saying that it may happen in other disciplines, but in their experience it does not, or not very often, happen in theirs.

### *Incommensurability and realism*

I tried very often to explain how Kuhn understood the concept of paradigm shifts by his own analogy of the conceptual shift that occurs between the different interpretation of the duck-rabbit or similar pictures. This in fact found a lot of favour as an analogy, and people agreed that it was a good way of thinking about the different world views that can possibly exist in science, even if they are otherwise quite fervent supporters of falsificationism, as for example respondent 7, who singles out the visual component of the analogy as what he finds appealing.

Yes, absolutely. I think that is a very good analogy. I think that's quite a nice graphical analogy. Vision is a very important part in science of course, because we do have to bear in mind that a scientist is not a, some sort of a perfect tool that doesn't have prejudices, that doesn't want to win the Nobel prize. (7 Senior, Biology, Male, UK)

Vision, and visual beauty played a huge role in the thinking of this scientist (see also his comments on simplicity in science, chapter 6).

Bound up with discussions about incommensurability were also some interesting remarks about the nature of what science discovers. The question of whether we believe that two theories can really ever be completely incommensurable was for many respondents bound up with what we believe about truth in science. Saying that scientific progress is dependent on the succession of incommensurable worldviews is often seen as incompatible with the belief that science approaches the truth. We cannot honestly say which of two paradigms is closer to the truth, or more likely to be true any more than we can say whether the duck or the rabbit is the correct interpretation of the lines on the paper. Whether this reading of Kuhn's philosophy as essentially anti-realist (or even stronger, as relativist) is correct is debatable. However, it has been a concern not just with some of the scientists I discussed that philosophy with, but also in more general scientists'



readings of Kuhn and sociologists of science inspired by Kuhn (see chapter 2 on science wars).

That this was an issue flagged up with regard to Kuhn's philosophy was often, though not always, due to my line of questioning, because I found it interesting to see how people who agreed with the paradigm concept regarded the issue of realism in science, and I found that there was indeed some genuine interest in the issue. Comments about realism were of course not limited to my questions on Kuhn, but often cropped up during the discussion of other topics as well, especially on the topic of Occam's razor (chapter 6).

There was no unbridled support for realism, and questioning beneath the surface I found a quite surprising array of opinions. Regarding paradigms and revolutions, the thought was often expressed that revolutions and paradigms do exist in science, but we can still say that the new one is better in explaining the world than the old one. This opinion is not in fact due to my possible misrepresentation of Kuhn, because it was held also by people who had learned about Kuhn in more detail, such as respondent 12. Here I asked about whether the new paradigm is not just a different way of looking at the problem and therefore not really more true.

There may be cases where that is true, that the new paradigm is no better than the old, it's just become a new fashion, I can believe that, I'm sure that happens, but science only advances if the new paradigm is better than the old one in explaining the way the world works. (12 Senior, Biology, Male, UK)

There was plenty of support for the argument that a revolution does not mean that old knowledge becomes obsolete, from all sides: people who vehemently disagreed with Kuhn, people who liked Kuhn's philosophy, and people who have only heard of Kuhn's philosophy through me. A typical response would be to take a revolution as an example, and argue that the science from before that revolution is still being used, as an approximation to modern science, for example classical mechanics. In that context that meant that the duck-rabbit analogy is reasonable but not the complete picture:

the sort of thing that you describe sounds to me more like the relationship between a negative and a photograph. That they're both potentially interpretations of what's there, and that's not unreasonable. But as time goes on, you find out more information, and therefore you can put the pieces together in a way that makes more sense than before and therefore you can move forward. It's like the world going from being flat to being round, you know, it's...(8 Senior, Biology, Female, UK)

There was also one scientist who argued that a paradigm shift can also represent a step backwards in at least some areas, but that this is an inevitable part of scientific progress:

Inevitably I think there are times when I think you know the paradigm shift has to take a step backwards. And inevitably that must happen every now and then. [...] You have a paradigm that is close to the truth and you throw it out incorrectly. But I'd like to think that in sum, the more information you have to throw at a problem, the better off you are. And so in sum, our understanding will improve. But I think it'll always be a little bit backward, a little bit forward, a little bit backward, a little bit forward, a little bit backward... (19 Mid Career, Chemistry, Male, UK)

This is possibly close to the idea found in Kuhn that sometimes the new paradigm is actually less well suited than the old to explain some phenomena, though this scientist was also very much of the opinion that science at least strives towards truth.

On the other hand there were rather surprising attitudes which are reminiscent of social constructivism, though in this case the scientist disliked that particular term. This comment was part of an answer to a similar question of whether incommensurability would mean that science does not discover truth:

Science doesn't discover the truth, it discovers *our* truth. [...] The sole purpose of science is to make images, and to try to understand very

complicated things with the simplest possible images. (29 Early Career, Physics, Male, France)<sup>25</sup>

### *Compatibility with falsificationism*

Following on from the issue of truth in paradigms, I often sought to portray Kuhn's philosophy in direct contrast to the hypothetico-deductive approach of progress in science, and in particular Popper. The incompatibility of the two approaches however was often questioned. Just as many respondents interpreted paradigm shifts to mean that there is a steady progression in science towards the truth, there was also a lot of sympathy for the idea that Kuhn and Popper were not in fact incompatible at all. This is a comment from a scientist who was quite fond of Popper:

I don't see the two are mutually exclusive, because, unless Kuhn says there's only paradigm change, or unless Popper says you can only do it via testing hypotheses, falsifying, and so on... [lets sentence trail off] (32 Senior, Physics, Male, France/En)

Paradigm shifts can be seen as arising from the accumulation of more and more falsifications. This was often the way my explanation of Kuhn was interpreted, and together with the fact that often "falsification" was not interpreted as strictly as Popper indicated. The idea of falsification was often combined with concepts on verifications, or with the idea that even falsified hypotheses are not irredeemable (see the section on Popper above). This means that many of the scientists could easily agree with both Popper and Kuhn at the same time. That was sometimes made explicit, even by scientists with more detailed knowledge of both:

[A revolution] is probably build up as, you know, a huge amount of falsification of hypotheses. Which forces people to think in a different direction entirely, so I don't think Popper and Kuhn in that respect... I mean,

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<sup>25</sup> La science, elle découvre pas la vérité, elle découvre *notre* vérité [...] La science ne sert qu'à se faire des images, et essayer de comprendre des choses très compliquées avec des images les plus simples possibles.

in their pure forms they may be [...] irreconcilable, but there are times in science when everything changes very radically. You can call those paradigm shifts after Kuhn, but I'm not sure what that means from a philosophy of science point of view. (12 Senior, Biology, Male, UK)

More often though there was a more implied understanding that both Popper *and* Kuhn had worthwhile things to say about science, and that these are not incompatible.

## **5. Discussion and summary**

### *5.1. Popper and Kuhn's ideas in popular science: Philosophers as authorities*

The fact that Popper received so much approval in the books (and to a lesser extent in the interviews as well) does not mean that there is an agreement on his philosophy. Also, just as there are authors who at least appear to follow philosophies that are quite at odds with Popper and who still call themselves Popperians, there may also be cases of people outlining a Popperian philosophy while denying that they are Popperians. One particular example, though not from my sample, is Feynman's famous (but probably apocryphal) assertion that "philosophy is as much use to a scientist as ornithology is to birds", coupled with his comment in his book *The Character of Physical Law*, where he describes science as being falsifiable, and that that "is all you need to know about scientific method" (Feynman 1965 p.156). It has to be noted though that in the famous quote on ornithology Feynman is not really anti-Popper, because he is anti-philosophy in general (at least where its practical usefulness is concerned).

However, in the sample, while there are certainly many references to theories or hypotheses being falsified, that alone does not say much about the author's idea about scientific method with respect to Popper. Many authors for example take on Popper's vocabulary of falsifying things, but then also talk liberally about verifying other things (see chapter 4). One possible exception is E. O. Wilson, who quite specifically remarks on falsifiability as a demarcation criterion.

Scientific theories, however, are fundamentally different. They are constructed specifically to be blown apart if proved wrong, and if so destined, the sooner the better. (Wilson 1998 p.56)

This comment is also remarkable because Wilson does not in fact mention Popper, even though in many other respects his book is philosophically very well informed. It may be tempting to conclude that Popper's influence on scientists has become so great that his ideas have become part of normal scientific discourse, that do not have to be specifically referenced any more (rather like the way that Deutsch would like it). This certainly appears to have happened with Feynman and Wilson. However, it also appears that Popper's victory then is somewhat pyrrhic: While the vocabulary that he introduced certainly made an enormous impact on scientific language (as also shown in the interviews), and while even he himself has become a signal of authority on scientific method, his actual philosophy seems to have had less success. As the first couple of quotes show, he gets mixed up with Nagel and the positivists. He also appears as an authority in books that otherwise disagree with his philosophy. At the same time, while his vocabulary is being used often, it is often either by people who then go on to espouse a contrary philosophy, or even, as in the case of Feynman, people may take on board his philosophy and then argue that all of philosophy is useless for science.

All this could be because, as Mayr has argued, his philosophy simply is not relevant for a scientist's (and certainly a biologist's) day-to-day life, so that people end up claiming to be Popperians while doing "what they want". But it can also signal how people understand what Popper stands for: because they hear so often that science proceeds like Popper says it should, people may come to think that whatever they perceive is happening in science must necessarily be Popperian. In a way the social representation of Popper's philosophy for this group could be analogous to the example Duveen (2000) gave of people's perception of the relative position of Vienna and Prague during the cold war (see chapter 3). People instinctively placed Vienna to the west of Prague because they were familiar with the fact that Prague is in eastern Europe, whereas Vienna is in western Europe. Similarly, the social representation of Popper's philosophy is *anchored* to the scientists' previous understanding of good science, to an extent that some of the

actual message of Popper's philosophy undergoes some changes. The unfamiliar concept (Popper's philosophy) gets conceptually attached to more familiar concepts (personal experience of science; a vague positivist understanding of it) and takes up some of its features. Within the new concept people look for familiar aspects, such as a commitment to rigour (for example Fortey).

Words like falsification or the principle of falsifiability as a demarcation criterion can almost unmistakably be traced back to Popper. For Kuhnian ideas this is unfortunately not so easy. The concept of a scientific revolution is very widely accepted even by people who do not agree with Kuhn, and those that have never even heard of him. The concept of scientific revolution was not one that originated from Kuhn, and phrases like the "Copernican revolution" have been used for a long time, for example by Kant (1993 [1781], preface). However, Kuhn's other famous concept, paradigm, is one he coined himself for this particular use at least for that particular context. But the word paradigm has taken on a general meaning that, again, many people do not necessarily associate with Kuhn. "Paradigm", with its general association with fashionable business-speak and cultural relativism has taken on negative connotations for many scientists. In many ways just like Popper, Kuhn can claim the pyrrhic victory that his concepts have spread widely, while his philosophy perhaps has not – with probably the added misfortune of having negative connotations attached to them in general scientific thinking. On the other hand there are clearly people who admire Kuhn as well as Popper, and are willing to defend their ideas in a popular forum, as well as during the interviews.

There may be something in the pattern of who admires Popper, and who admires Kuhn. Popper seems to have been used as a respected authority figure across the disciplines, with maybe the slight favour towards the physical sciences. Two of the three authors who praised Kuhn are computer scientists, which is even more striking considering that these two were the only books on computer science in my sample, though this may well be a coincidence. As for the third, Buchanan, though he himself is a physicist, his book (on networks) is largely multidisciplinary and touches on computer science on many occasions. Finally I have to remark on Steve Grand: When I was drawing up my guidelines on who in the Aventis-prize shortlists I should count as a scientist (see chapter 1), Grand was certainly the most

borderline case of them all. He does not have a doctorate, and apart from some honorary research fellowships, he never worked in academia or industry as a scientist (see his website (Grand 2004) for biographical information). I decided to count him in because he has published in peer-reviewed science journals<sup>26</sup>, and because the work he does is clearly seen as scientific and respected as such by scientists (regarding his lack of qualifications, in the acknowledgments he thanks Richard Dawkins for advising him against studying for a PhD: it would be a waste of his time – Grand 2003 p.ix).

With computer science being something of an outsider in the sciences, and with one of the authors even being a self-confessed outsider himself, their defence of Kuhnian ideas seem more interesting. With its popularly perceived emphasis on revolutions in science, Kuhn's philosophy certainly lends itself naturally to outsiders in science (this is not quite what Kuhn had in mind himself though since he also emphasised the importance of working within the paradigm most of the time). This is something very easily observable with authors who are certainly not respected within the scientific community and defend themselves loudly by proclaiming their opponents cling on to outdated paradigms, or who claim to herald the next scientific revolution. A typical example of this usage of Kuhn's ideas would be the "intelligent design" community (mentioned in Lambert 2006).

Also worth quoting here is another one of the authors in my sample, David Horrobin, who is certainly not considered to be part of the scientific mainstream. Horrobin is among the originators of the fish-oil and evening primrose oil industry, which has received a lot of criticism lately, and faces serious accusations of being unscientific (see my overview of the books in chapter 1). While Horrobin did not launch himself into a defence of his ideas by referencing to Kuhn's authority, he did call his ideas "unconventional" (Horrobin 2001 p.216), and portrays his career as one of a continuous struggle against the scientific community, which "had a paradigm to work with" (p.160). This interpretation may be somewhat unfair to Grand, Buchanan and Naughton, as they are all respected scientists in their own right, and certainly not comparable the intelligent design community, and have not been in a controversy to the one that surrounded Horrobin.

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<sup>26</sup> For example Grand & Cliff (1998)

In summary, the use of specific philosophers (Popper and Kuhn and also occasionally others like the positivists or Ernest Nagel) within the popular science books shows that the philosopher is very often borrowed to provide legitimacy to the author's point. This use of the philosopher as an authority figure may be somewhat surprising, given the often enough muttered sentiment that scientists do not care for philosophers' opinions. But there seems to be slightly more to it: Instead of just being an authority on scientific method, the philosopher can also be used to signal the author's philosophical identity. Thus it does not matter what Popper really stood for, other than "scientific rigour", and therefore showing to the readers (and to other scientists, and of course to yourself) that your following of Popper also shows that you are rigorous and therefore a good scientist. The philosopher can also act as a legitimating authority for more unconventional ideas, as is the case with Kuhn. Here that may be a signal that the author is (or even perceives him or herself as) something of an outsider, but at least the same has been shown of other respected scientists in the history of science. What is more, a respected historian and philosopher (Kuhn) has pointed the fact out that the outsider in science can often succeed and that the establishment has been wrong in the past.

There are also the books that discuss philosophy extensively, like Deutsch and Mayr (and, to an extent, Wilson). Although in these cases I do not wish to see too much in their more extensive and critical evaluation of these philosophers, I think it is important to wonder what drove these authors to write philosophical critiques for the mass market in the first place. For these authors philosophy is something that has to be engaged with seriously and to be discussed with a public audience. That does not mean that the author has to be enthusiastic about philosophers in particular – but that they think it is important to have these ideas discussed. I would therefore put even the prominent science wars books of Wolpert (1992) and Weinberg (1993) in a similar category, because all these books worry about the problems of the nature of science and about what the public should know about it. Whether they actually endorse professional philosophy is in a way secondary as they are all themselves philosophical works. The rhetorical work that the philosophers in the books perform can be categorised as authority figures and as identity markers.



## *5.2. Reputations predisposing reaction, and philosophers as boundary objects*

I have argued above that some of the reason that authors like Deutsch are prepared to identify themselves with Popper's philosophy while still disagreeing with it to some extent, may signify the adoption of Popper as an identity or boundary marker which shows the author conform to what they think is proper scientific attitude, i.e. supporting Popper. In the interviews this is evident as well, though somewhat weaker, with respondents who reacted very positively towards Popper, but then also disagreed with his philosophy, or at least argued that there must be more to science than just falsification. Some of them also argued, similar to Deutsch, that surely Popper would have agreed with them, or that he did not really mean it like that. Throughout the interviews the interviewees also often qualified their remarks by saying that they were not well qualified to speak on these philosophical matters (see chapter 4), which in some sense also gives the authority to speak of such matters back to the philosopher.

With regard to Kuhn, the results are different, partly because he himself is less well known, and partly because his philosophy is seemingly more at odds with the typical ideas of scientific method that scientists have, as shown in chapter 4. In the books he gets criticised by two scientists, one who is a Popperian and one who argues against most philosophers, but otherwise he does get his fair share of praise as well. In the case of the scientists who defend him, I argued that this may be due to the fact that the authors represent whole disciplines that are more outsiders and sometimes seen as less scientific than physics or biology. It certainly does seem the case that outsiders from science frequently use ideas of revolution and unconventionality to justify their ideas. This may be similar in the books, as shown above.

In the interviews I have found a lot of sympathy for the ideas of revolutions and paradigms, which may be surprising considering how many people were also supporting a hypothetico-deductive idea of science and some even a straight Popperian one. An acceptance of revolutions happening in science is itself not incompatible with Popper's philosophy, because the general concept of revolution in science is not unique to Kuhn. However several times people thought of Kuhn's philosophy in particular as not necessarily incompatible – both as a reaction from my own attempts to explain Popper's and Kuhn's philosophies, but also from

people already familiar with them. In these discussions about the compatibility of the two philosophies, people replied that they think that there is maybe a good way of reconciling the different approaches to science, and that you may well believe in both kinds of characterisations of science. In a way then, the treatment that Kuhn received is somehow the opposite of the one Popper received (at least very prominently in the books): While Popper's celebrity and well-known status as a defender of science may predispose some scientists towards his philosophy, the relative obscurity of Kuhn, (even when he is regarded by many science warriors as harmful) may in fact have helped *not* to predispose scientists against his ideas in the way the science warriors would have us believe they should.

In this sense, I would argue that the reputations that some philosophers have predispose the scientists' reactions to them: Popper's reputation is one of being a proper scientist, and therefore his ideas get interpreted in a favorable way. Kuhn on the other hand is not quite as well known within the scientific community for his slightly negative image to have a similar effect. Where Kuhn does get discussed favourably in the books, his authority had to be augmented by arguing for his ideas more at length, or by calling him a historian, or remarking how obvious it is.

I have argued that the scientists' representations of Popper's and to a lesser extent Kuhn's philosophies are shaped by the expectations and images that people have of the philosophers. This can take the shape of the philosopher becoming a marker of a scientist's philosophical identity which may be somewhat removed from the scientist's actual philosophical opinion, as I have shown especially with respect to the representation of Popper in the books, though this identity aspect is much less in evidence for Kuhn than it is for Popper.

Next to being identity markers, Popper and Kuhn can also be seen to function as boundary objects. As I set out in chapter 3, identity markers in social identity theory can be interpreted as a kind of social representation, and I have added my own argument that Star and Griesemer's (1989) concept of boundary object can also be possibly understood as a social representation. In this case it can be argued that the philosopher is also a boundary object, especially in the rhetorical role as authority figure whose arguments need no further explanation. In the case of the authors at least, the imagined readership (both public and other scientists), will

recognise the famous philosopher endorsing the author's point of view, and will therefore accept it. The philosopher thus becomes, in Star and Griesemer's phrase a "common coin" between the groups (scientist and reader) even if the different groups' conceptions (or representations) of that philosophies are slightly different.

Kuhn, as I have suggested, fulfills a slightly different role, as his more controversial standing is openly acknowledged (similarly here to the concept of reductionism, see chapter 7). Yet he is still needed as an authoritative boundary object that the author can use to position himself scientifically and convey that message to the reader. As I have argued above, an identification with Kuhn's philosophy could give scientific credibility to disciplines and individuals that are more on the margin of what is properly accepted science.

Unlike Popper or Occam (see chapter 6), however, Kuhn can have problems being recognised as speaking authoritatively for scientists (due to his reputation, as shown in the science wars). Two things therefore have been done in the books to increase Kuhn's authority: Firstly, his philosophy gets explained in more detail, and in one case at least, the conclusion is reached that it was obvious all along. In this way, it is shown and explicitly argued that Kuhn's philosophy applies to science, and that he therefore is an authority on it. Secondly, as I have argued above, Kuhn is twice referred to as a historian rather than a philosopher, which could possibly be seen to elevate his writing from mere philosophizing to observed, historical fact.

### *5.3. Summary*

One of the themes from the previous chapter, on the philosophy in the books being used for boundary work, gets developed further as an identification of Popper and maybe even Kuhn as embodying good scientific practice, with which the author will identify. At the same time the philosopher as an authority becomes a boundary object himself. Particularly Karl Popper has become so widely identifiable as embodying good scientific practice that many scientists feel their interpretation of how science works must accord to Popper's philosophy, and it therefore gets interpreted accordingly. In a way therefore the reputations that philosophers have predisposes the reaction that scientists have towards them, and how they interpret the philosophical content of what those philosophers have written. Popper's philosophy often gets interpreted in the best possible light according to the

scientists' own experiences or opinions. Kuhn's philosophy gets used when science is more on the margins of being properly accepted, as his philosophy can be seen to argue that even unaccepted science may in the end come good. Interestingly, while Kuhn is often maligned for a philosophy that is seen as too relativistic, as shown in often during the science wars, in the interviews scientists have often held views surprisingly sympathetic to Kuhn's philosophy – some scientists knew the concepts only through my exposition of them, but there were also several scientists who were already familiar with Kuhn. This therefore shows a similar effect to the one on Popper, but from a different perspective, i.e. that Kuhn's relative obscurity means scientists were more receptive to his ideas, even if a lot of the science wars literature argued against him. The role of a philosophy becoming an identification for scientists will be further looked at in chapter 7 on reductionism, where the philosophical concept has become an object of contention between two rival scientific groups.

At the same time, the philosopher can also be seen as providing a common reference point between scientists and the public, or between different scientists. Because there are so many interpretations that can reasonably (and in some cases unreasonably) be called Popperian or Kuhnian, the philosopher and his philosophies can act as a shared representation or boundary object which two different groups can use, in their own ways, to make sense of the rest of the communication – as with the pithy remarks from chapter 4, these characterisations can serve to underline the scientificity of a concept by pointing to the great philosopher.

## Chapter 6: Occam's razor and simplicity

|                                                                                           |     |
|-------------------------------------------------------------------------------------------|-----|
| <b>1. Introduction</b>                                                                    | 176 |
| <b>2. Philosophical introduction to Occam's razor</b>                                     | 178 |
| 2.1. Usual philosophical introductions to Occam's razor                                   | 178 |
| 2.2. What is simplicity?                                                                  | 181 |
| 2.4. Three types of Occam's razor                                                         | 182 |
| <b>3. Occam's razor in the popular science books</b>                                      | 184 |
| 3.1. Occam's razor as an authoritative rule of distinguishing science from non-science    | 185 |
| 3.2. Simplicity, elegance and aesthetic as a feature of good science and the world        | 188 |
| 3.3. Dissenting voices                                                                    | 191 |
| 3.4. Discussion of the uses of simplicity in the books                                    | 195 |
| <b>4. Occam's razor in the interviews</b>                                                 | 196 |
| 4.1. The ontological razor and intuitive evaluations of simplicity                        | 197 |
| 4.2. Explanations for simplicity and the epistemological razor                            | 200 |
| 4.3. Criticisms of Occam's razor: The merely methodological razor and outright rejections | 203 |
| <b>5. Discussion and summary</b>                                                          | 205 |
| 5.1. Occam's razor vs the principle of simplicity                                         | 205 |
| 5.2. Relevance of simplicity for science                                                  | 207 |
| 5.3. Implications for philosophy                                                          | 208 |
| 5.4. Summary                                                                              | 211 |

### 1. Introduction

The principle that in science the simplest option is usually the best, often called *Occam's razor*, is frequently seen as a central part of scientific method. This can be seen from many of the more informal statements in the popular science books, and I have also heard similar sentiments in some of the interviews. Outside of my sample, some of the most famous and influential popular science books have featured Occam's razor in their explanations of science. Discussing the idea of Laplace's demon<sup>27</sup>, Stephen Hawking, in his famous and influential popular science book *A Brief History of Time* argues that "It seems better to employ the principle of economy known as Occam's razor and cut out all the features of the theory that cannot be observed." (Hawking 1988 p.61). Carl Sagan gives Occam's razor an even more incisive role as part of his famous "baloney detection kit" (Sagan 1996

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<sup>27</sup> A hypothetical being which can observe the state of every single particle in the universe and can therefore calculate the future, in a deterministic system, see Gillies 2000

p.198) which is meant to help the reader identify “fallacious or fraudulent” arguments. Sagan’s use of the term in fact fits quite well into the way the authors in my sample have written about Occam’s razor.

Occam’s razor also seems to be a widely accepted principle in philosophical literature. Many philosophers, including some very influential ones such as Karl Popper have written about Occam’s razor fairly extensively. Next to that, Occam’s razor has become a feature of many discussions in computer science and especially artificial intelligence and decision theory<sup>28</sup>, Bayesianism<sup>29</sup>, and other more technical subfields of science and philosophy.

Very often, specifications of Occam’s razor extend to other arguably related concepts than simplicity, such as parsimony, elegance, aesthetics, symmetry or beauty. Arthur Miller for example argues that beauty and aesthetics play a crucial role in science (Miller 1996), and beauty was the uniting theme of a popular science collection of essays on “great equations of modern science” – titled: *It must be beautiful* (Farmelo 2002). In the absence of a clear definition of what simplicity is, I will also follow this popular usage of Occam’s razor and take discussions of these terms as being part of the discussion about Occam’s razor. Most often these concepts and simplicity are very much seen as related, and a number of interviewees independently made that connection themselves without my prompting.

Although I will concentrate in this chapter on the specific term of *Occam’s razor*, I will also be discussing the value of *simplicity* in science. In the absence of any widely accepted specific formulation of Occam’s razor that is more precise than “simplest is best”, I will assume that “Occam’s razor” and this “simplicity principle” are philosophically the same thing. That does not mean that people actually talk about the value of simplicity in science and about Occam’s razor in the same terms, as I will show later on in this chapter. Some scientists, though they are not numerous, may not have heard of the term Occam’s razor, but will nevertheless have opinions about the philosophical content of it, the value of simplicity for science. Similarly in the books, the authors on the whole tended to write differently about Occam’s razor when it is named as such, and about the value of simplicity or similar concepts, such as the value of aesthetics, symmetry, beauty in science.

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<sup>28</sup> For example Domingos 1999

<sup>29</sup> Myung and Pitt 1997

This chapter will show that scientists, both in the books and in the interviews, have talked and written differently about Occam's razor than they have about simplicity. While Occam's razor in many ways fulfilled a role that was similar to Popper in chapter 5, as an authoritative philosophical boundary object which easily defines what is and is not science, simplicity gets much more critical and philosophically insightful attention. This shows the authoritative position the philosopher can have, and supports my argument of Occam as a boundary object as well as an identity marker that signals, like Popper, that the scientist is properly scientific in his views (though in this case it is about excluding what is not science which differs from the positive demarcation work mentions of Popper were mostly doing by signifying things as properly scientific).

At the same time, I will show the philosophical opinions regarding simplicity and its value among scientists are very varied and nuanced. This will be contrasted to the way philosophers have discussed simplicity and Occam's razor, in that I will categorize the scientists' opinions in a way in which it is rarely categorised in the philosophical literature. This shows, similarly to the overall philosophical stances surveyed in chapter 4, that the scientists in this study think about these philosophical problems in different ways and ordered into different categories than philosophers traditionally do. This chapter will therefore, more so than the preceding one, also include possible lessons for philosophers themselves.

## **2. Philosophical introduction to Occam's razor**

### *2.1. Usual philosophical introductions to Occam's razor*

I would like to suggest some of the difficulties that are associated with the principle itself, in an effort to clarify what it actually says, what are the philosophical consequences that its acceptance can have for the structure of the world, and of science, or both.

In the interviews I have introduced Occam's razor usually like this: "*When you are faced with two theories (hypotheses, statements etc) that, other things being equal, both describe the available evidence equally well, is it sensible to choose the simpler one?*" This rather vague formulation captures the essence of what the principle is generally held to say, and through its vagueness gave plenty of room to the respondents to interpret the principle as they like it best, and possibly echoes the

vagueness in most philosophical introductions to Occam's razor like the one shown from Kelly, below.

A different difficulty with introducing Occam's razor, as with many traditional philosophical principles, is that there is considerable confusion about what it actually means (see also the philosophical introduction to reductionism in chapter 7). When scientists introduce Occam's razor, the principle is often quoted as the maxim that "entities should not be multiplied beyond necessity", and attributed as a direct quotation from the mediaeval philosopher William of Ockham (also often spelled Occam). Some authors even like to quote it directly in Latin: "*Entia non sunt multiplicanda, prater necessitatem*". This particular formulation however does not appear to have ever been used by Ockham himself, and has therefore even been called "the myth of Occam's razor" (Thorburn 1918).

The simplicity principle was not an invention of Ockham himself. A similar train of thought has long been present in Aristotle's philosophy, which in turn had a huge impact on scholastic mediaeval philosophy. Ockham is usually introduced as a nominalist, a school of philosophy which held that there are no universal or abstract concepts that have any independent existence (Spade 1999). As such Ockham's and other nominalists' championing of the simplicity principle makes sense, because it holds that we should do away with unneeded abstract entities. Note however, that this nominalist ontology may counterintuitively necessitate the speculation of more, not fewer, discreet entities, thus going counter to how the principle is often interpreted today: Since for Ockham there can be no such thing as a universal "whiteness", there are as many whites as there are white objects (Spade 1999).

Nominalist philosophers before Ockham, such as Robert Grosseteste and Duns Scotus, have derived the simplicity principle from Aristotle, (see also Rodriguez-Fernandez 1999). What distinguished Ockham from the other scholastic philosophers who used the principle was that he used it most extensively, making it one of the cornerstones of his logical system for which he became famous (Spade 1999). The term "Occam's razor" itself is relatively recent, having first been used in the 19<sup>th</sup> century by William Hamilton (Hamilton 1852, noted in Thorburn 1918). Spade (citing Brampton 1964 and Maurer 1984) notes that Condillac had earlier in the 18<sup>th</sup> century used the expression "nominalist razor" to refer to the simplicity principle used by the nominalists in general, which may have influenced Hamilton.



Most modern usages of Occam's razor are also slightly different from the original simplicity principle as used in the philosophy of Ockham himself, as I will explain below when I introduce the different ways of interpreting the principle.

Contemporary philosophical works about Occam's razor or simplicity very often stress the intuitive character of the principle and then proceed to look for possible ways of formally describing what is meant by it. Elliot Sober, for example, prefaces his book on simplicity by remarking that "[t]he diversity of our intuitions about simplicity is matched only by the tenacity with which these intuitions refuse to yield to formal characterization. (Sober 1975 p. vii)". The rest of the book concerns his efforts to find such a formalisation and of how it applies to both philosophy of science and science. Similarly, the Stanford Encyclopedia's entry on simplicity (Baker 2004) starts with the sentence: "Most philosophers believe that, other things being equal, simpler theories are better", though it features a general discussion on why that should be so which I found lacking in Sober.

Another typical example of a philosopher's introduction to Occam's razor is in Kevin Kelly's paper on Occam's razor:

One of the deepest puzzles in the philosophy of science concerns simplicity. When several, possible theories are compatible with experience, scientists incline toward the simpler one, where simple theories are somehow more uniform, symmetrical, unified, or free from independently adjustable parameters." (Kelly 2004).

Note that in both these cases the mere intuitiveness of Occam's razor is enough for the philosophers to try to find ways of formalising it.

The intuitiveness of the principle is indeed one of its major attractions, and this usually entails that philosophical discussions about Occam's razor centre around the questions of how to spot which one of two competing hypotheses is the simpler one, and about how the principle can be fitted into a general philosophical framework. Thus Sober's introduction to his own view of simplicity centres on identifying several often contradicting ways of how simplicity can be defined and then examining the virtues of the proposed alternatives. Other works, such as the many works on Occam's razor in Bayesianism, game theory, and artificial

intelligence detail how the principle works within the framework of that particular theory. Yet others such as Anderson (2002) relate how the principle has worked with an example taken from actual science, in this case geology, which happens to be Anderson's own discipline. Although I believe these accounts of Occam's razor all serve a useful purpose for their own scientific or philosophical discipline, I will not concentrate on what they have to say about its actual definition in these works.

## *2.2. What is simplicity?*

One of the central questions in philosophical discussion on Occam's razor centres on the question of what exactly we mean by simplicity. Historically there has been a rather confusing array of definitions which even include the contradictory opinions of simplicity as high probability and simplicity as low probability, along with the many interpretations of simplicity that do not feature probability at all (Sober p.vii). In technical applications of the razor, like the ones in decision theory and artificial intelligence mentioned above, the principle can achieve rather complicated ideas of simplicity that fulfil the formal criteria set by the philosopher so that the principle performs the desired task.

In practical applications relevant for scientists, a problem arises that what is simple can often be seen as a subjective matter rather than an objective one that could give a direct answer to a scientist stuck in a theory choice dilemma. This is not always a problem, as some scientists interpret Occam's razor to be an aesthetic principle rather than a logical one. If, like Popper and the majority of philosophers, we reject this interpretation of the razor, we run into difficulties very quickly.

Suppose we have the choice of two lines that both fit the data, a straight line and a circle. Aesthetically, both are equally simple. Mathematically, the straight line seems easier to describe, unless of course we switch to polar coordinates. It appears therefore that what is simpler is often dependent on background assumptions of the scientist mulling over it. Philosophical discussions such as that of Popper (who attached simplicity to the concept of testability), and Sober (who defines simplicity as informativeness) have provided formalised solutions to such problems, though I will not be discussing them here for the very practical reason that the more complex and refined the definitions and discussions get about what exactly is simplicity, the more it loses its intuitive appeal.

It is ironically precisely the most sophisticated philosophical definitions of simplicity that are in danger of being ignored by scientists, because they are too complicated. This will not matter in specialised contexts such as finding epistemological justifications or for designing rules for reasoning in artificial intelligence, where most recent discussion of Occam's razor has taken place. It does however affect its usefulness to actual scientists whose supposed intuition has started the whole question in the first place. Therefore, even if we find an epistemologically satisfactory answer to the problem of simplicity, it may well fail as a methodological, normative principle for scientific practice.

The traditional formulation which I have given above is open to interpretation on other points as well: what do we mean by "other things being equal"? Can we really imagine a case where *all* other things really are equal? How do we decide what the other things are that matter, and which ones do not? Here there could be disagreements over how important the other things can be, and how we decide on it. Presumably the consideration of who formulated the theory and when should be irrelevant, and therefore not one of those "other things" that need to be equal. So, intuitively, there are some things that do not have to be equal, and where exactly we set the dividing line is again more one of subjective preference (or possibly due to social norms), than a strictly logical choice. And even if such a logical choice is found, it will presumably suffer from the same problems as that of the definition of simplicity itself – it will be too complicated for the intuitive character of Occam's razor to survive.

#### *2.4. Three types of Occam's razor*

With respect to the role that Occam's razor has variously been supposed to play in science, and the justifications for it, I propose to classify it into three categories. These three categories are meant to chart the possible usefulnesses of Occam's razor, they are not meant to be mutually exclusive, as it is perfectly possible that Occam's razor has several uses or justifications.

Baker (2004) distinguishes between epistemic justifications (it is rational to believe the simpler theory) and methodological justifications (it is rational to adopt the simpler theory "as one's working theory for scientific purposes") of Occam's

razor. To this I will add a third type of justification, a variation of Baker's epistemological one, which I have come across on several occasions: It is rational to believe in the simpler theory because that is what the world is like.

#### *A) The methodological razor*

Under this application of Occam's razor, it is more a practical matter that we should choose the simpler option. I call it "methodological", with Baker, because it prescribes Occam's razor as a *method* for doing science. If there really is no other reason to choose one way or the other, why should the scientist make his/her already stressful life harder by choosing the complicated option?

While nobody suggests that the scientist should go for the more complicated option, this justification of the value of simplicity for science is speedily dismissed by some philosophical commentators such as Popper: As Popper is generally looking to find a logic of science, purely practical considerations are "extra-logical" (Popper 2002 [1934] p.122), and therefore not quite what he is looking for when he tries to define simplicity and why or whether it is a good idea. A purely methodological razor is somewhat unsatisfactory from a normative philosophical perspective. The injunction that the scientist should go for what is easier depends on the personal preferences of the individual scientist (or, again, on social norms), and can therefore not serve as a normative, logical philosophical principle.

#### *B) The epistemological razor*

Instead, it is frequently argued that the scientists' intuition to prefer the simpler hypothesis must rest on an understanding that it is the best way to proceed for reaching an understanding of the world. This for example is Popper's position, as he argues that simplicity is an intrinsic value of a good scientific hypothesis. In fact, because Popper defines simplicity to be nothing other than the degree of a hypothesis' testability, it is one of the cornerstones of Popper's methodology, getting a whole chapter to itself. Note that this does not depend on an understanding that the world will eventually turn out actually to be simple – it merely supposes that by consistently choosing the simpler hypothesis, we will gradually get a better understanding of the world whether it is simple or not.

The same goes if we suppose simplicity to be an approximation. It can be argued that a hypothesis is simpler because it is only an approximation, much in the same way that “3” is a simpler value for pi than “3.141”. In this sense of simplicity, we have the almost counterintuitive result that the simpler a hypothesis, the further away it is from a faithful description of the world, but the more likely it is to be true.

These two contrary ways of identifying simplicity also lead to the quite incredible fact noted by Sober above, that the simplicity in Occam’s razor has both been identified with being probable (the simplicity-as-an-approximation idea), and with being improbable (Popper’s idea of simplicity-as-testability rests on the idea that the more likely something is to be true, the less falsifiable it actually is).

### *C) The ontological razor*

Here we assume that fundamentally the world is simple, and because it is simple any successful scientific theory must mirror that fact. This idea has a considerable intuitive appeal for some people, and has been justified through the argument of the uniformity of nature, which underlies the principle of induction. If we try to find generalisable laws of science from induction, then we will have to assume at least some sort of uniformity of nature. If we observe a pen to fall ten thousand times, then the simplest assumption is that it will fall the ten thousand and first time as well.

Going further than mere inductive reasoning, however, the ontological idea of simplicity is one that pervades such fundamental quests as the search for the “theory of everything”, as it is in essence trying to explain the world with as few forces and particles as possible. In a similar way often the ideas of beauty, elegance or aesthetics of a scientific theory betray a conviction that the world is in some way easily understandable and the truth pleasing.

### **3. Occam’s razor in the popular science books**

Occam’s razor was one of the most frequently mentioned philosophical concepts in my sample of popular science books, together with comments that discuss the value of simple (or elegant or aesthetic) explanations without mentioning Occam’s razor directly. The fairly frequent usage of the razor for demarcation work shows that it is considered as quite important when communicating what science is, and that it does

indeed offer a very neat and easy way of dismissing cranky theories and ideas that the author feels are not worth discussing in more detail. Another way of talking about simplicity in science however emerges when the authors talk about its value within science. Here, concepts such as elegance, beauty and aesthetics are invoked, the underlying principle is explained in more detail, or it even pervades the whole philosophical message of the book, and curiously (with one exception) neither Occam himself nor his razor are mentioned. Finally, even though the principle has this intuitive appeal to many of the authors quoted below, there are also some who do not think Occam's razor is always a good thing, and are prepared to argue against it.

### *3.1. Occam's razor as an authoritative rule of distinguishing science from non-science*

A typical reference to Occam's razor is to discredit the more wild and wacky type of theory that the author needs to argue against. This can be something like a general conspiracy theory, or a pseudo-science that needs to be shown to be such, like astrology or creationism. One particular example of this usage of Occam's razor is Webb (2002). In this book, Webb discusses the "Fermi paradox" (if aliens existed, why are they not here?), a question attributed to a remark by the physicist Enrico Fermi (Webb 2002 p.17). Because of the subject matter, which is basically all about discussing some of the more outlandish proposals made by all manner of people about the existence of extraterrestrials, Webb has recourse to Occam's razor on a number of occasions. Its first appearance is in a discussion of the theory that aliens are keeping us unwitting humans in a simulated environment. Webb remarks that:

Occam's razor gives us a good reason for rejecting all these [theories]. Suppose you throw a ball and watch its parabolic path: you will conclude the ball is an autonomous object obeying Newton's law of gravity. The alternative - that some system (whether an individual consciousness or a sophisticated virtual-reality generator) contains laws that simulate the properties of the ball and its motion under gravity - is a more complex explanation of the same phenomenon. Both explanations fit the observations.

But Occam's razor tells us to use the simplest explanation, which in this case is that the ball is "real." It has an autonomous existence. We can make the same argument regarding our observations of the Universe. (Webb 2002 p.53)

Although this is a popular science book aimed directly at non-scientists, Webb assumes a basic familiarity both with the principle, and with its explanatory usefulness. The invocation of Occam's razor is enough for the author to discount these theories – no explanation, no introduction. After that passage, Occam's razor occurs again at several places throughout the book, and always in similar places, where the author needs to argue against something obviously too complicated.

E. O. Wilson fits his discussion of Occam's razor into a passage that discusses the nature of scientific theories, and therefore goes more naturally into a discussion of the principle, without assuming that the reader is already familiar with it. The message however remains the same: we use Occam's razor to discount the more fantastic stuff:

The best theories are rendered lean by Occam's razor, first expressed in the 1320s by William of Occam. He said, "What can be done with fewer assumptions is done in vain with more." Parsimony is a criterion of good theory. With lean, tested theory we no longer need Phoebus in a chariot to guide the sun across the sky, or dryads to populate the boreal forests. The practice grants less license for New Age dreaming, I admit, but it gets the world straight. (Wilson 1998 p.56)

Parsimony lets us discount the supernatural explanations from mythology; Occam's razor is therefore an important tool in science.

Using Occam's razor can give us an indication of why some possible explanations are so intuitively bad. Richard Fortey, for example, wonders about why, when both birds and bats have wings, everybody can agree that they are two completely different types of creatures in almost everybody's classification scheme. The answer is our intuitive grasp of Occam's razor.

Our common sense appreciates that bats are flying mammals rather than furry birds. What this natural nous does intuitively is to apply Occam's razor to the question of the ancestry of bats. We recognize more characteristics linking bats with other mammals than bats with birds; it seems a simpler and preferable arrangement to assume that Nature would have to make fewer alterations to turn an insect eating mammal into a bat, than to turn a bird into a bat. This simple guiding principle is what William of Occam (fl. early fourteenth century) is said to have determined: given the choice, we prefer an explanation that is simpler. (Fortey 2000 p. 159)

These three passages share a common theme about Occam's razor, one that is very common generally in popular science. Occam's razor is seen as arising from common sense: this is particularly strong in the passage from Fortey, but also in the others. Webb does not require an explanation of Occam's razor, presumably because it is so intuitively right that even a lay audience will understand it immediately. Wilson may have felt more of a need to explain why science needs Occam's razor, but even he does not go beyond intuition. This passage indicates that for Wilson, simplicity might be more of a subjective and even aesthetic judgment.

Scientists attempt to abstract the information into the form that is both simplest and aesthetically most pleasing – the combination called elegance – while yielding the largest amount of information with the least amount of effort. (Wilson 1998. p.57)

Seen in this light, as a scientists' way of applying common sense or our intuition, Occam's razor is not really a criterion for rational theory choice between two seemingly equally plausible contenders, as the problem is often discussed in philosophical literature. Instead it is a crude but rhetorically powerful demarcation criterion between science and the unconventional, fantastical or plain wacky ideas of theories that are definitely not science. It is in this respect a perfect philosophical tool for scientists' boundary work in Gieryn's original sense of demarcating science from whatever is not science (chapter 3). Outside of my sample, this use of



Occam's razor crops up very often in popular science. One particular very famous example is Carl Sagan's "baloney detector kit" introduced above:

What sceptical thinking boils down to is the means to construct, and to understand, a reasoned argument and, especially important, to recognise a fallacious or fraudulent argument. (Sagan 1996 p.197)

He then proceeds to give a list of tactics and rules to spot such fallacious argument. Among them is Occam's razor:

Occam's razor – this convenient rule-of-thumb urges us when faced with two hypotheses that explain the data *equally well* to choose the simpler (Sagan 1996 p.198, original emphasis)

There is however some confusion here over the precise role that Occam's razor is supposed to play for Sagan: This item in Sagan's baloney detector kit will not give a user any tool for deciding if an argument *on its own* is fraudulent or fallacious, because we would need an alternative hypothesis to compare it with. While the baloney detection kit is described as a demarcation tool for deviding science and non-science, the way Occam's razor is defined is as a tool for theory choice *within* science. Though these two approaches are not mutually exclusive, they are not the same. Specifically for Sagan's purposes, i.e. for spotting if an argument is fallacious, if there is no other argument to compare it with, Sagan's version of Occam's razor is no help.

In any case, even though it may turn out to be much less useful for the practical purposes of demarcation, in popular science discourse Occam's razor plays an important part in the boundary building and demarcating between science and pseudo-science, fantastic ideas, and baloney. Through the invocation of Occam's razor, the author can easily show an idea to be unscientific and even mock it as almost ridiculous: the device is supposed to be uncontroversial, and often the reader is expected to be already familiar with the term.

### *3.2. Simplicity, elegance and aesthetic as a feature of good science and the world*

Authors have also often talked about simplicity or elegance of a scientific theory or experiment without mentioning Occam's razor, and without necessarily performing any demarcation work to distance science from "baloney". Often this took the shape of the writer mentioning that a particular theory was good because it was simple or elegant. For example, in an epigraph Marc Buchanan approvingly quotes Stanley Milgram: "Simplicity is the key to effective scientific inquiry" (Buchanan 2002 p.27). It is not only theories that benefit from simplicity: experiments, too, are good if they are simple. Armand Leroi remarks about a particular experiment that

[t]he experiment [a particular scientist] carried out [on salamanders] was of such elegance, simplicity and daring that seventy years later it can still be found in textbooks. (Leroi 2003 p.145).

Mentioning the simplicity or elegance of some theory or experiment is of course quite pervasive in popular science. In these cases simplicity gets invoked to explain why a scientific theory or experiment was better than another scientific theory or experiment, rather than why something is scientific at all or not. What these kinds of accounts have in common though with the above demarcation-type discourse on Occam's razor, is that simplicity or elegance is seen as something desirable in its own right, rather than merely making the job of the scientist easier.

Though in many cases this kind of quote is too short to reveal if that is really is what the author meant to say, for one author at least the whole tone of the book is geared towards arguing that the universe has to be simple. Brian Greene's book even took a title that suggests this. *The Elegant Universe* (Greene 2000), hints at the underlying theme of simplicity and elegance as something vitally important for science. String theory, the subject of the book, deserves to be considered because of its simplicity and elegance, even if it blatantly fails in some other traditionally held criteria for good science, such as testability. Similarly to Wilson, aesthetic judgments feature in Greene's explanation of why simplicity is so sought after in science:

The elegance of rich, complex, and diverse phenomena emerging from a simple set of universal laws is at least part of what physicists mean when they invoke the term "beautiful." (Greene 2000 p.169)

Greene suggests that the universe is in effect simple and elegant, and that any successful theory about it should reflect that fact. This is much more than merely suggesting that simplicity is good because it makes the job of the scientist easier, this is a philosophical conviction that the simpler theory is really more likely to be true than the less simple – an ontological razor in my classification.

Generally, celebrated scientific quests such as the search for a “unified theory of everything” suggest the importance of a particular type of simplicity (that of explaining almost everything with just one theory), and any book on such a theory must almost necessarily have simplicity as an underlying theme. Because it is about a similarly fundamental physical concept, that of time, Davies’ book also has such a grand, pervading feel of simplicity. Though unlike Greene, Davies does mention the razor itself, he uses similar language about the aesthetics of science: He talks about a “beautiful symmetry” in Maxwell’s equations, which “also serves to make electromagnetism simple and elegant” (Davies p.158). Here he talks about Einstein’s famous cosmological constant  $\Lambda$ :

Taking their cue from the great man, scientists in general have tended to regard the  $\Lambda$  term as being as repulsive as the force it describes. Partly this is a reaction to Einstein’s dramatic U-turn, partly it is because of Ockham’s razor. Why add an extra term to a set of equations that is already demanding enough? It only serves to multiply the choice of cosmological models on offer and obscures the interpretation of astronomical observations. (Davies 1995 p.157)

Unlike the other authors who mention the razor quoted in section 3.1, Davies here gives a longer explanation of what the razor says, and why we should follow it. Remarkably, even though Davies describes the principle as an undisputed fact about science, and the simplicity, elegance and beauty of scientific theories are very much an underlying theme to his book, the principle itself turns up in a discussion where

its application has been premature, because the cosmological constant has actually not been the mistake that Einstein thought it was.

A train of thought similar to that which underlies Greene's and Davies' theme of beauty and simplicity is behind this quote from computer scientist John Naughton:

Elegance? It may seem odd to non-scientists, but there is an aesthetic in software as there is in every other area of intellectual endeavour. Truly great programmers are like great poets or great mathematicians - they can achieve in a few lines what lesser mortals can only approach in three volumes. Paul Dirac's PhD dissertation ran to only nine pages, but in them was distilled the essence of quantum mechanics. (Naughton 1999 p.172)

Unlike Green, though, Naughton does not make any assumptions about the nature of the world, but rather about the nature of good science: Simple or beautiful science does not mean that the world is simple or beautiful.

The two broad approaches to Occam's razor I outlined here, the demarcating-science-from-baloney use and the theory-choice-within-science use are of course not mutually exclusive. If you believe that Occam's razor can tell you which scientific theory is the better one for a scientist to pick, then you could also use it to distinguish it from stuff that you want to argue is altogether not science at all. They also say nothing much about your philosophical justification of Occam's razor. As the comparison between Greene and Naughton shows, one can defend Occam's razor from several viewpoints, either that the world is simple and that good science therefore must be simple, too, or that the world may or may not be simple, but good science is, what I have argued above is the distinction between the ontological and the epistemological razor.

### *3.3. Dissenting voices*

In the books quoted above, the authors generally tend to give the impression that their views on simplicity and its value are uncontroversial. While there might be

some indication of uneasiness about the particular philosophical justification for it, simplicity on the whole is seen as a good thing, and every scientist is portrayed as being in agreement with that.

This however, is not quite the case. First of all, just as there are plenty of comments about some theory being good because it is simple, there can also be comments about some theory being bad because it is *simplistic*. Moreover, rather than that being just a throwaway comment that an author has made without realising what it means for Occam's razor, it can be argued as an explicit refutation of the idea that beautiful, simple or elegant is always best. Robert Weinberg for example, while discussing an early (and ultimately wrong) theory on the mechanisms of cancer, observes that

[h]ere was a clear example of how research in molecular biology could uncover a hidden mechanism of great simplicity – so simple and logical that scientists would describe it as “beautiful.”

But some thought the scheme was much too simple. They even called it simplistic, and implied that those who believed in it were intentionally ignoring much that was known about cancer formation. (Weinberg 1998 p.55)

It has to be remarked here that Weinberg's comment on the unreliability of simplicity is not throwing doubts on the principle itself but on human fallibility: people were ignoring evidence for sake of their simple theory. Other things were not equal. This however is still in direct contrast to the value of simplicity for people like Greene: for Greene's cosmological theory simplicity is so important that it at least temporarily overrides testability and maybe even initial negative results.

One astronomer however goes further and principally questions not only the usefulness but also the underlying rationale of Occam's razor. Kirshner's 2002 book, the very title of which, *The Extravagant Universe*, seems to be a direct challenge to Greene's famous *The Elegant Universe*<sup>30</sup>, argues that the world is far too weird a place that a principle advocating simplicity should be helpful. Theoretical physics is

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<sup>30</sup> Greene's book was very influential and widely read, so it must be very unlikely that the title is a coincidence.

accused of trying to fit the data of a complicated world into a too simple theory. Why should Einstein, for example, have felt the need to apologise for the introduction of the cosmological constant when it turns out he might have been quite right about it after all? (Kirshner 2002 p.58). Discussing the case of dark matter he remarks that

A neater universe crafted by Occam's razor might have just one form of dark matter, but our extravagant universe apparently must have at least three: some dark baryons, a pinch of neutrino mass, but mostly something else. Instead of a minimalist universe, we seem to live in a rococo one: we have everything you can think of, and more than you can think of. Perhaps we should not be so quick to use Occam's razor to reject wild ideas: we need even wilder ones to interpret these startling results. (Kirshner 2002. p.129)

One possible way of making sense of the philosophical divide between Greene and Kirshner is to see this as a conflict between theorists and experimentalists, especially since Kirshner himself seems to set up the division like this, arguing consciously against “the theorists”. It would certainly make a lot of sense to suppose that a theorist, especially on a large-scale science like cosmology or fundamental particle physics, would see the discovery of an underlying simplicity to the universe as a great triumph. It also makes sense that an experimentalist, who knows that in real life things can actually get quite messy, sees this as almost misguided naivety. This is certainly a hypothesis that I considered worth investigating further during the interviews. I did not, however, find a significant difference between theorists and experimentalists when I talked to people (see below). It may well be that theorists in material science or chemistry think differently about the structure of the world than particle physics or cosmology theorists, in which case I just did not talk to enough people like that to reach a conclusion.

In my sample of the popular science books, however, one theoretical physicist expresses at least reserved skepticism towards Occam's razor. In generally very philosophically opinionated book, David Deutsch explains his reservations about Occam's razor by observing that what is being judged as simple is dependent on

who does the judging and his or her background assumptions. Deutsch also gives his own preferred definition of the principle and quickly explains why it should be valid.

We may cite the principle of Occam's razor: 'do not multiply entities beyond necessity' - or, as I prefer to put it, 'do not complicate explanations beyond necessity', because if you do, the unnecessary complications themselves remain unexplained. However, whether an explanation is or is not 'contrived' or 'unnecessarily complicated' depends on all the other ideas and explanations that make up one's world-view. (Deutsch 1997 p.78)

Having introduced Occam's razor in this slightly skeptical manner, Deutsch proceeds to explain his criticism by showing how the inquisition's opposition to Galileo was not as irrational as commonly thought, because a razor like argument can be made against Galileo's theory itself. This argument would then seem plausible because of the initial unquestioned assumptions the inquisition made.

Despite such reservations though the principle is still useful and is made use of by Deutsch to argue against solipsism.

[T]he general refutation that I have given of such doctrines shows us that it is irrational to build upon that possibility. Following Occam, we shall entertain such theories when, and only when, they provide better explanations than simpler rival theories. (Deutsch 1997, p.137)

The general attitude of Deutsch towards Occam's razor thus seems to be that even though it is not always clear how to make the judgment on which theory really is simpler, there *is* an objective good way of finding that out, and we do not have to resort to aesthetic judgment. When the principle has failed it was not the fault of simplicity as a guiding principle: it was human fault again, in this case unquestioned assumptions were made. This is also reminiscent of the "sociology of error" argument developed by sociologists of scientific knowledge (Bloor 1976), according to which scientists invoke social factors when things somehow go wrong but explain successes with an empiricist view of science.

### *3.4. Discussion of the uses of simplicity in the books*

In the usage of simplicity/Occam's razor, I have identified broadly three different types of occurrences: It can be used as a demarcation criterion between science and non- or pseudoscience; it can be seen as a criterion for theory choice or generally good science which is used to distinguish between different scientific options; lastly, of course, the author can be skeptical towards the whole idea. Here there are certain parallels with the way Popper and Kuhn are represented – the philosopher as a person is a source of authority that is used to close off an argument. Similarly also there may be an element of the author anchoring Occam's razor to what they already know about scientific practice. Occam's razor, like Popper, has such a good reputation, that the actual philosophical principle gets conceptually combined with what the scientist knows about scientific practice. There may therefore be an element of scientists accepting Occam's razor as a principle, while in fact having more nuanced opinions on the actual philosophical substance of the principle of simplicity. This possible discrepancy does not really show itself in the case of Occam's razor, as the people who claim to follow it do not give much of a definition of what they actually think it means. However, I will show that this is actually the case when the authors discuss reductionism, see chapter 7.

I have also identified a parallel disagreement of opinions about whether Occam's razor says anything fundamental about the world, or just about science. Here actual opinions are harder to see, as many comments on simplicity or Occam's razor do not give away too much. But there is a discernable difference between the value of simplicity for Greene and that for Naughton: For Greene the world has to be simple, for Naughton it appears that it is only science that needs to be simple. Even among the people who have reservations about the value of Occam's razor in science a similar division is visible: While both Deutsch and Weinberg seem to be objecting more to the practicality of using it – in both their examples the problem with the rule is down to human error rather than the way the world is structured – for Kirshner the rule is naive precisely because we can not be sure whether the world is in fact simple.



There is an interesting pattern as well about who uses the term “Occam’s razor”. In the cases above where Occam’s razor has been used to do demarcation work for science, the authors never dispute it, and it is talked about as if every scientist is supposed to agree. In these circumstances the principle usually appears with the name “Occam’s razor” attached. Notice also that in the one book where Occam’s razor is mentioned but does not perform any obvious demarcation work, Davies’s book on time, the principle is nevertheless seen as undisputed. In the second category however, where the principle underlines a conviction about the aesthetic nature of science and the world, and where it appears as a principle for theory choice, Occam’s razor is rarely mentioned by name. When the authors wished to argue that something was good because it was simple, just saying that would usually do, the author had no particular need to mention the name of the principle – even when, as in Greene’s book, a version of the principle was central to the whole message.

One way of interpreting this is that it is important for the boundary-work use of simplicity to be associated with some authoritative statement from a (relatively) well known philosopher to back it up. Belief in fairies and government conspiracies is unscientific because it complicates a hypothesis. Don’t just take my word for it, no, it’s an accepted piece of philosophical wisdom dating all the way from the 14<sup>th</sup> century. Like this the argument gains in authority, and through the assumption that every scientist agrees, effectively cuts short the need to argue *why* simple should be better. Here, like Popper and Kuhn (chapter 5) the philosopher fulfils the role of an authority figure.

In the more mundane, everyday use of the word “simple”, like the one quoted from Leroi, “simple” of course is more a synonym of good, and without too much emphasis placed on it, should maybe not be taken to be indicative of the author’s thoughts or intentions. However, even in short appearances like in the epigraph quoted by Buchanan or Naughton’s praise of the shortness of Dirac’s PhD thesis, simplicity can take on a defining importance for science.

#### **4. Occam’s razor in the interviews**

Because the interviews formed part of my larger project I usually started the discussions by asking the respondents what they think is science (see chapter 4). In

a small number of cases, the subject of Occam's razor came up spontaneously in that discussion. If that was not the case, I would usually formulate the simplicity principle as described above in the introduction to Occam's razor. In a number of cases, the interviewee would then respond by explicitly mentioning that this is Occam's razor.

#### *4.1. The ontological razor and intuitive evaluations of simplicity*

The fact that Occam's razor, or more rarely some mention of the importance of simplicity have sometimes come up spontaneously in the conversation without being directly prompted by me, indicates that at least for some people, Occam's razor is centrally important to their view of science. In several cases the principle formed an integral part of scientific method for the respondents.

Scientific method is that within some system you make hypotheses, you make testable hypotheses, you test them, and you keep the ones that are verified, and you throw out the ones that are falsified. If you have two hypotheses which are verified, which explain the same phenomenon, you use Occam's razor and take the simplest. (32 Senior, Physics, Male, France/En)

However, further discussion often revealed that people are only confident that at best they have merely a "gut-feeling" that this is the best way to do science, and that they do not really see any particular reasons why a simple hypothesis should be better. Also, we cannot believe that Occam's razor helps us in uncovering the truth if we do not believe science is in the business of getting the truth. When I asked the scientist quoted above whether he thinks that a simpler hypothesis is more likely to be true he remarked that

I don't really think anything is true, that's the problem I've got, because I mean I get a bit, I get a bit allergic to absolutes, so... the... there's no doubt that it's a practical thing, *at least*, but is it only this, is the question I guess. (32 Senior, Physics, Male, France/En)

If nothing else, Occam's razor here seems a practical consideration. Its epistemic worth is not really being questioned, but it is at least sidelined. In a similar vein, some people reacted positively to the idea that simple is better, but recognize that they do not know why, and that they would struggle to find a reason for it.

Occasionally the initial reaction I got from asking the question would be very positive, even if the scientist had not explicitly made Occam's razor part of his initial answer to the question on scientific method.

Occam's razor. Very, very important, absolutely critical. And you must always favour, not accept, but favour, the hypothesis that is the simplest hypothesis consistent with the facts. That's, fundamental in scientific... fundamental. (7 Senior, Biology, Male, UK)

Similarly to the above respondent, this scientist remarked in the subsequent discussion that he would not be able to explain why Occam's razor should apply. When I asked whether a simple hypothesis is more likely to be true, he hesitated before ultimately responding that it is, but that this opinion rested merely on a "gut feeling".

*Respondent:* ...simplicity is more likely to be true... but don't quote me on that [*laughs*].

*Me:* Well, if you ask me not to...

*Respondent:* No, you can say what you like, I mean that facetiously. Just I couldn't stand up in front of, you know, various philosophers and defend that remark. (7 Senior, Biology, Male, UK)

There was also a sense among some respondents that we know that simple is more likely to be true, but we just do not know why. The following remark also came up in an unprompted discussion of Occam's razor: "I wish we could work out why mathematical beauty is a good guide to the universe. (2 Early Career, Physics, Male, UK)". Note here also that mathematical beauty is seen as the simplicity that Occam's razor is about.

What I call the ontological razor therefore seems to have at least had some followers among the interviewees. While there was a general consensus that we can not really prove it, some people had a very strong feeling that it somehow is right. This argument was sometimes backed up by the argument that for us to be able to do science at all, the universe *has* to be understandable. If nature was not uniform and therefore in some sense simple, science would be impossible. Similar to the popular exposition by Greene of the search for a Grand Unified Theory shown above, there were also some comments that explained the quest for that theory of everything with the search for simplicity (though curiously it wasn't always positive towards either).

That is, we search for a theory of everything because it's a theory that explains everything, and that [*inaudible*] simple. (33 Senior, Physics, Male, France)<sup>31</sup>

People also very often spoke about the beauty, aesthetics or elegance of theories, similarly to the way it was discussed by Greene or Naughton.

I find that in my case, that the... quite often that the beautiful things are the correct things, you know, and that the chaos, the rubbish is also, is usually wrong. Because something's gone wrong with it. And this really, you know is, this... I have affinity for this, because I like.... I like beautiful things. (18 Mid Career, Biology/Physics, Male, UK)

However, there was also a sense in which the intuitiveness towards Occam's razor precluded the principle's epistemological application. As long as there is such a strong *intuitive* feel that simple is best, we may as well trust our intuitions and not get bogged down with any precise calculations about which of two hypotheses is marginally simpler. After all, if we have to go through lots of effort to find out what really is simpler, the intuitiveness is going to be lost.

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<sup>31</sup> C'est, on cherche la théorie de tout parce que c'est une théorie qui explique tout, et ça [*inaudible*] simple.

#### *4.2. Explanations for simplicity and the epistemological razor*

Some respondents ventured to find an explanation of what simplicity other than an intuition or a gut feeling, while others provided arguments why they think it cannot be done. I see this response as a version of the epistemological razor, because there is a mirroring of the philosophers' traditional preoccupation about Occam's razor. Also, it does not necessarily say anything about the world as such. In fact, some of them had a rather negative initial reaction to Occam's razor. This respondent asserts that he sees

no particular reason why the simplest should be the one that's true. I mean I can imagine lots and lots of ways to make life easier, it doesn't mean they're true. (23 Early Career, Physics, Male, UK)

However, he does venture an explanation of how simplicity could be defined:

And actually my PhD supervisor always used to say, on this aspect of agreeing with data, he's only interested in the first factor of two. Get within the factor of two, and then, and then the interesting physics ends, and somebody else can work out the details. So what I think he means by that is that you, you want an idea that explains broadly lots of things to a reasonable degree of accuracy, rather than one idea that, that explains one thing to a billionth of a percent. (23 Early Career, Physics, Male, UK)

Here we have a variation of the idea that simplicity is defined as an approximation. Therefore, even though Occam's razor is recognised by the respondent as being valuable for science, it does not in fact entail that the underlying world is simple. A similar idea of simplicity as an approximation was also made by other respondents. Others combined this interpretation of simplicity with their aesthetic judgement. Respondent 33 for example, whom I quoted earlier as saying that simplicity is a motivation for the search for a theory of everything, also holds that the physicist often takes the more simple hypothesis, because it is mostly an approximation, which will make everything look more aesthetically pleasing.

A common identification of the meaning of simplicity was that of testability, occasionally even specifically identifying Popperian falsifiability as the meaning of simplicity. One respondent who initially reacted very guardedly towards Occam's razor, admitted that how you define simplicity is difficult. Nevertheless on reflection gave this idea of what simplicity could mean, and why it is useful in science:

I like the simplicity for that matter, I like the Darwinian natural selection principle, it's very beautiful you start with one idea and it explains many things. (9 Senior, Physics, Male, UK)

This idea of simplicity is in fact quite similar to Popper's sense of falsifiability: the more things one idea predicts, the more easily it can be falsified. It has however to be considered also that despite Popper's dismissal of simplicity as a practical consideration as "extra-logical", even scientists who agreed with his idea that "simpler is more falsifiable" did not necessarily see that as a logical choice, but as a practical one, because a more falsifiable theory makes life easier: The line between simplicity as a practical consideration and logical choice may not be so clear as Popper has argued.

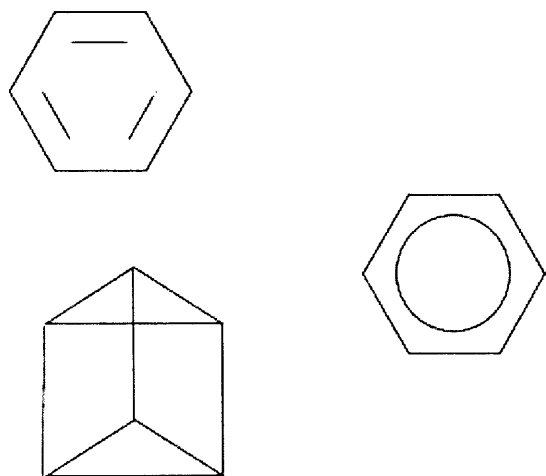
Another related idea was to identify simplicity with the number of "degrees of freedom", or parameters.

There, I want to say that... I myself see it like, I think it's, maybe a, there is an analogy with the number of degrees of freedom. [...] It's the number of independent parameters that you have for explaining a dataset. (35 Senior, Biology/Physics, Female, France)<sup>32</sup>

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<sup>32</sup> La, je veux dire que... je le vois moi comme un, je crois un, peut être, il y a une analogie avec nombre de degrés de liberté. [...] C'est le nombre de paramètres indépendants que vous avez pour expliquer un système de données.

Quite often though, people reacted spontaneously questioning the whole principle of Occam's razor, precisely because it is hard to define what simplicity actually is. One respondent gave a historical example and drew three alternative versions that were discussed at the time of the structure of benzene on a piece of paper, challenging me to say which was simpler (see figure 1).



**Figure 1: Three possible structures of benzene - which one is simpler?** (Mid Career, Chemistry, Female, UK)

Another has pointed out that it's the background knowledge you have that determines what you find is simpler.

I think it really depends on what's the audience. We are confronted with that problem on a practical basis all the time, because you're trying to explain, let's say a result, and you try to do it in the most simple way, two people have very different levels of knowledge, so simple theories are great for people who have a more elementary understanding of the world or what you're trying to explain. (34 Mid Career, Physics/Chem, Male, France)

#### *4.3. Criticisms of Occam's razor: The merely methodological razor and outright rejections*

One of the most common responses was that Occam's razor is useful, but for practical reasons. As the quote above from respondent 32 shows, the practicality of Occam's razor was a common fall-back position even of people who thought that there was more to the principle than that. Many scientists went further and argued that this is the only reason to prefer simpler hypotheses.

Just as the practical choice argument was often made by people who thought there was something more to simplicity but could not say exactly what, even those who argued that simplicity was easily definable and gave such a definition argued that at the very least that's why we go for it. Curiously however, even people who completely disagreed with Occam's razor often made the same argument. While Occam's razor has misled us in the past, and may even be harmful to our understanding of the world, it still makes no sense to go for the more complicated one.

There was however a group of people for whom just the practical aspect of the razor was enough to agree with my statement of Occam's razor that it is reasonable to go for the simpler hypothesis, but then also argue that that isn't always the case, either.

[Occam's razor] tends to make good biological sense, because it is less resource wasting to use a simpler way of doing something than a complex way of doing something. Although that isn't always true, and in certain circumstances you find multiple pathways that actually achieve the same thing, because it's so important that if that didn't happen, then life would cease, or whatever. But... on, you know, on sort of a day to day basis it usually holds true, I would have thought. (8 Senior, Biology, Female, UK)

However, not everybody reacted positively towards Occam's razor, even in the limited sense of the methodological razor. One respondent was talking about the difficulty he had with general definitions of scientific method, as they didn't give him any practical, useful hints on how to proceed when faced with radically



different worldviews. Here he is talking about the difficulty he faced when explaining to a friend why he should not believe in creationism:

I mean, I don't buy Occam's razor, either, particularly, I mean it's something you use, but it's not very useful when you're comparing various, different paradigms. (21 Mid Career, Physics, Male, UK)

Another possible reaction was to question not whether it is sensible to choose the simpler one, but whether we should choose at all. This actually tied into the respondent's beliefs about the value of scientific realism, and therefore led her to reject the razor as a practical guide on what to believe, if not on what to work with. If all things really are equal, why do we *have* to choose at all?

well if there is sufficient evidence for both, then I believe both happily. And then wait for more... I mean if the experimental data doesn't favour one of them, why should I believe the simpler one? [...] I probably would because it's easier to understand, easier for my brain to work on, but if they both show sufficient data, then either of them might be right or neither of them might be right, so I'm happy to believe both until one is disproven. (25 Early Career, Biology, Female, UK)

This respondent's idea of truth in science is unusual, but in fact not too different from respondent 32 quoted at the beginning of section 4.1, who by contrast was quite enthusiastic about Occam's razor.

One argument was that Occam's razor only gets us to the next best solution for a problem, without regard to the overall correctness of the approach itself. Therefore Occam's razor is very often irrelevant to the advancement of our understanding, and possibly harmful as it leads us up blind alleys.

So all you do is you take small steps in the right direction. And to me that's what Occam's razor is like, is that you say, this is the, this is... I'll go in this direction... doesn't mean you end up at a, a what's called a global minima,

you end up in a, most likely, local minima. And to me that's analogous to people having different paradigms, basically, you know, you can't get from one paradigm to another one, using Occam's razor. [...] So you would have thought maybe Occam's razor would keep you trapped in that little corner (21 Mid Career, Physics, Male, UK)

An even more extreme reaction is to argue that Occam's razor may even be *harmful* to our understanding of the world, as it is maybe more wishful thinking rather than leading us to the truth:

But at the end of the day, if you want to understand that phenomenon or something completely, probably going a little complex would help, because it would [...] the simple thing of action towards just something that you want to believe it's true. (22 Early Career, Physics, Female, UK)

Here the argument is similar to that of Kirshner who argues that the world is actually a complex place. If we want to understand it, our science also may have to be more complex.

## **5. Discussion and summary**

### *5.1. Occam's razor vs the principle of simplicity*

A number of popular science books, among them some of the most influential, such as Hawking and Sagan, use the specific mention of Occam's razor as a universally acknowledged scientific principle which closes off the debate by appeal to philosophical authority, in effect saying that "this is the way things are done in science". Occam's razor performs the rhetorical work of boundaries, not really between the science and the public, but between science and pseudoscience, because mostly Occam's razor is used to argue against the more fantastic, seemingly ad hoc hypotheses. The actual status of Occam's razor in science, where it derives its validity from, and what could possibly be meant by simplicity are issues that are rarely discussed. Even one of the authors in my sample who discuss Occam's razor more critically, David Deutsch, acknowledges at least the principle is epistemologically valid, because his only criticism is when it is being misapplied.

Contrasted with that we have the authors who discuss simplicity, its role in science, and what it says about the world. Here curiously we do rarely get people mentioning Occam's razor by name.

In the interviews a slightly different story about Occam's razor unfolds. While for some people, Occam's razor forms an integral part of scientific method and can therefore possibly be used as an intuitively obvious demarcation principle between science and non-science, this view was not held very often, and was quickly qualified in the discussion as either meaning that it does not really have any real implications for getting at the truth as such, because that concept is itself highly problematic, or that its intuitiveness, the "gut feeling" it conveys is all we really have to justify it.

Often people who offered me an explanation of what simplicity could mean and how it could be useful in science, did not really like talking about "simplicity" because it is such a vague concept. This is similar to Popper's argument (Popper 2002 [1934] p.131; addendum to the 1972 edition), who gives his definition of simplicity as testability, but then argues that he does not want to get bogged down on whether this really is the definition that best captures our intuitions of simplicity, and instead wants us to concentrate on the epistemic benefits of *testability*.

The opinions on the nature of science also did not always match professed agreement with Occam's razor: A number of people in the interviews initially reacted positively towards Occam's razor but then offered an explanation of what could be meant by simplicity for the principle to become valid; an opinion which is not that much different in fact to some of the people who *disagreed* with Occam's razor or the simplicity principle, but then also offered an explanation of what simplicity could be for the principle to be valid. Possibly in some cases the difference between people who accept Occam's razor and those who reject it is one of emphasis rather than actual opinion. Taking testability as an example, people could either say that "testability is simplicity, therefore Occam's razor is valid", or they could say "Occam's razor is invalid, because we should be talking about testability instead of simplicity". In both cases the actual opinion about scientific method may actually be the same, though one accepts Occam's razor, and the other rejects it.

Where I think the similarities lie between the popular science books and the interviews is that there is a difference about how people talk about Occam's razor and how they talk about simplicity. While Occam's razor often is an authoritative device to be used for boundary work in the popular science books, in the interviews where it found a lot of agreement it usually got qualified when the discussion turned to simplicity itself. Simplicity on the other hand, both in the popular science books and in the interviews, gets a good amount of critical discussion which reflects a surprising amount of variation of opinions.

Another thought to consider is the way the "razor" metaphor may have influenced the way the principle came to be seen as a demarcation tool. The original use of the metaphor to designate the simplicity principle came either from Hamilton or possibly Condillac, but in any case was for a long time not part of the way the nominalists or Ockham in particular described their principle. The razor metaphor probably came about because the principle "shaves off" unnecessary metaphysics from a theory, however, this shaving metaphor also suggests a precise, sharp instrument which the nominalists probably never really claimed to have – Occam's razor is a very blunt tool due to the many different variations of what simplicity should mean and why we should use it in the first place. It is possible that if the principle had been named "Occam's club", or if the metaphor had been one of ripping out unnecessary metaphysics – or if it had just been called "the simplicity principle" – these associations may have produced different interpretations of it. In particular the demarcation of science from non-science aspect that people seem to associate with Occam's razor but not so much with simplicity may be partly due to the use of the metaphor (see Brown 2003 and also Ortony 1993 for a collection of essays on metaphors in science, featuring one by Thomas Kuhn).

## *5.2. Relevance of simplicity for science*

If we then take the matter of actual agreement or disagreement with Occam's razor aside and consider the pattern of how the scientists rated the use of simplicity, and what simplicity is about in the first place, I would class people by using my own terminology of methodological, epistemological and ontological razor. While the ontological razor was held by a few people, all of whom nevertheless felt unable to

justify it, this was very much a minority position. Much more common was an epistemological type of opinion, where the scientist argued for a particular interpretation of simplicity: testability, approximation, degrees of freedom, likelihood etc. This however was also often, though not always, accompanied with a denial that *simplicity* has any role in science, and therefore not necessarily an endorsement of the simplicity principle. If we need to look out for testability or probability in science, then let's look out for that, rather than letting us be guided by some otherwise vague idea such as simplicity. Then there were a number of people who argued against simplicity, saying that the principle can lead to harm and even that they would believe both options happily if they have no other way of choosing. There still prevails a sort of methodological razor, because nobody would voluntarily work with the more complicated option, but that does not say anything about the world or how science should logically operate.

I have not seen any particular pattern in the spread of these opinions. I found that respondents' ideas about simplicity were pretty unpredictable across discipline, career stage, gender and which country I held the interview in. That there were no noticeable patterns in this regard with regard to the discipline I found particularly surprising. I have found fairly big differences of opinions even between people working in the same laboratory, while the most vocal supporters of Occam's razor came from astronomy, surface physics, and oncology. Following the disagreement in the popular science books between Kirshner and Greene, I also looked for any difference not between disciplines, but between the theorist/experimentalist divide within the same discipline, and again did not see much of a pattern.

### *5.3. Implications for philosophy*

The main difference between the popular science books and the interviews is not the spread of opinions but their numbers. A majority of the books discussed Occam's razor as something positive, which is uncontroversial in science. Even those who discussed simplicity more extensively tended to side with Occam's razor. This was either as an ontological razor in books like that of Greene, or the epistemological razor in Naughton. Even those who disagreed with the practicality of Occam's razor like Deutsch hold an ontological view of simplicity, and argue only that it has on many occasions been misapplied. Only Kirshner stands out as a

critic of Occam's razor. In the interviews, while there certainly were some who agreed with the ontological razor, most people did not. Even some of those who initially reacted positively towards Occam's razor turned out to favour a merely methodological razor. Scientists who were prepared to give some value to simplicity also often rejected the principle itself as unhelpful. In all, though the principle was not rejected out of hand, this response was tepid at best, and this is hardly a ringing endorsement for a principle which is supposed to be intuitively held among scientists. This by the way is not merely a popular belief with philosophers, but also among the scientists themselves – a lot of interviewees, even many of those that rejected the principle or were skeptical towards it, told me that they thought the principle was widely held by scientists in general.

While I will not argue that there is no possible debate to be had about justifying the use of Occam's razor, there are fundamental questions about its applicability that simply are not debated, or the answers taken for granted. As I showed above, even book length monographs on simplicity start with the assumption that the principle is intuitively held by many scientists, and build up from there. True, if the simple question is asked whether the simplest option is best (methodological razor), then Occam's razor will be supported by the majority of scientists. However, probing deeper into how the question is interpreted reveals on many occasions an understanding of Occam's razor which is far removed from that of philosophers.

An endorsement of Occam's razor which extends to a mere methodological razor is unhelpful for underpinning the philosophical debates surrounding the principle. The methodological razor is indeed intuitive, but philosophically rather trite. What the methodological razor classes as simple is whatever the scientist concerned finds easier to work with, and this varies according to the individual scientists' preferences, abilities and background knowledge, or it may vary according to disciplinary standards. Indeed, it may even be at odds with the same scientists' notion of epistemological simplicity, as voiced for example by respondent 35 above.

Ontological razors can possibly ground philosophical discussions because of the intuitiveness with which they are held. However, any ontological razors that rely

on aesthetics or beauty are self-consciously idiosyncratic. Philosophically attractive would be an objective and intuitive simplicity, something like the uniformity of nature principle. But then there is the other drawback of this option for the philosopher, which is that the ontological razor was not actually articulated by most of the scientists that I talked to, not even a large minority, and therefore it is apparently not quite as intuitive as it is made out to be.

Most promising for the philosopher would be the epistemological razor, which however also raises a question. As it seems to have been noticed by Popper himself, if we manage to give a definition of simplicity such as testability, it is almost certainly better to talk about testability rather than simplicity. Otherwise we run the risk of circularity: what is the reason to prefer talking about simplicity rather than testability other than that simplicity is an intuitively good thing to have? We cannot use that if we want to find a philosophical or logical formulation of simplicity that is designed to justify that intuitiveness. Popper wisely avoids that by toning down his identification of testability with simplicity, saying that testability is only one aspect of simplicity, the only one that he found to be logically analyzable (Popper 1992 [1963] p.241). Since his concept of testability may not conform to all intuitions about simplicity, he added the following in an addendum to his chapter on simplicity in the *Logic of Scientific Discovery*: “Nothing depends on the word ‘simplicity’. I never quarrel about words, and I did not seek to reveal the essence of simplicity” (2002 [1934] p.131).

In this discussion I do not seek to contribute on whether Occam’s razor can or cannot be justified logically or philosophically, I merely wish to point out that any discussion of Occam’s razor that takes its obviousness as a starting point risks begging the question. This may not be such a bad thing as it sounds for philosophy of science, because most discussions do in the end move on to the technical aspects of the various definitions and justifications for simplicity that they are advocating, at the end leaving the situation much like Popper who distances himself from simplicity as such. But to what extent then does it still make sense to talk about Occam’s razor, or to point out that it is intuitively held by scientists?

#### *5.4. Summary*

In this chapter I have looked at one of the more pervasive philosophical principles found very often not just in the popular science books in my sample, but also in some of the most widely read books in the genre. Also, the principle was well recognized, both under the specific name “Occam’s razor”, but also under a more vague understanding of a “simplicity principle” during the interviews. I have given a slightly longer philosophical introduction in this chapter due to the fact that the philosophical literature on the principle is rather sketchy. In particular, the philosophical literature on the principle was rather unhelpful for me in interpreting the remarks about it from the scientists, and I have therefore tried to distil the philosophical debate about the types of Occam’s razor into three distinct categories, with which I then analysed the scientists’ responses to the principle as well as reevaluating the philosophers’ own justification for debating the principle at all.

Within popular science there are two distinct discourses on the principle, one concerning Occam’s razor as a demarcation tool between science and nonsense, the other as an almost metaphysical speculation on the value of simplicity for science, and the world. Similar divisions were visible in the interviews, although here scientists were more thoughtful about the implications and justifications for simplicity in science. In particular, while most scientists agreed that it is sensible to go for the simpler hypothesis, the precise justification for why they said that is often unsatisfactory for philosophical purposes.

I develop further the argument from chapters 4 and 5 that the philosophical principle can be used for the boundary work by the authors, in this case mostly to demarcate science from nonsense. Here however, the role of the famous and authoritative philosopher for that demarcation becomes much more pronounced as the actual philosophical ideas behind Occam’s razor, simplicity and its use in science, gets discussed in a fairly different manner to “Occam’s razor”. Because here there is so much difference between the way the philosophy gets discussed in philosophy, popular science, and the interviewees, I have also thought about the possible implications for philosophers, scientists and the study of popular science; this theme will also be revisited in the next chapter on reductionism, where there are similar differences, this time even within popular science.



# Chapter 7: Reductionism

- 1. Introduction .....212
- 2. Philosophical introduction.....216
- 3. Reductionism in the popular science books .....218
  - 3.1. The life sciences .....219
  - 3.2. Situating reductionism within the Nature/Nurture debate .....224
  - 3.3. The physical sciences .....233
- 4. Reductionism in the interviews .....235
  - 4.1. Scientists’ own definitions of reductionism.....236
  - 4.2. My exposition of reductionism and reactions towards it .....242
- 5. Discussion and summary .....252
  - 5.1. Reductionism as a philosophical identity.....253
  - 5.2. The value of reductionism for the disciplines and its implications for the development of philosophical debate within science .....257
  - 5.3. Implications for popular science.....259
  - 5.3. Summary.....261

## 1. Introduction

The topic of reductionism was prevalent in the popular science books, having been mentioned more than other philosophical topics. Next to being a frequent topic in popular science, it has received a lot of criticism especially in the social sciences because for many people it seems to imply that all of the social sciences “reduce” to no more than physics, and that this is neglecting and simplifying the bigger picture (one particular example relevant here are social identity theorists, Hogg and Abram, introduced in chapter 3, 1988 who remark on p.13 that “social psychology’s crises can be attributed to its reductionist theorizing” and that social identity theory is a good thing because it is non-reductionist).

On the other hand, practitioners of astrology, homeopathy, and many other practices that scientists often regard as pseudo- or even fraudulent science, have also often argued against reductionism. In this context then, reductionism has found itself in the firing line of an altogether bigger dispute between scientists and “pseudoscientists” where the scientists frequently imply that being against reductionism is almost being against science, and this is reflected in the way reductionism (and its antonym, holism) was and is being debated during the science wars. Where the social scientists found themselves in the firing line of scientists

wishing to attack pseudo-sciences, their rejection of reductionism was well noted. (Steven Weinberg 1993 for example argues for reductionism in his famous popular science contribution to the science wars.)

The previous two chapters started by stating what a casual glance at popular scientific discourse predicts of scientists attitudes towards that philosophy, and in this case, it seems that scientists should pretty unanimously side for reductionism, if the claims from the science wars literature are correct. However I found that in the popular science books that I sampled there is much more of an acceptance that reductionism is a controversial topic (compared for example to the almost unanimously enthusiastic responses towards Occam's razor, or Karl Popper in the books), and that even though most authors accepted reductionism, they mostly show an awareness that the topic is controversial and not necessarily self evident, an awareness that was absent with Occam's razor and Popper, though not with Kuhn.

Regarding the philosophy behind reductionism, just as was the case with Occam's razor, I found that much of the philosophical literature was slightly unhelpful for the interpretation of the scientists' remarks on reductionism. I will therefore concentrate on ideas that I found reflected in the popular science books. Reductionism has given me problems of a different nature than have Popper, Kuhn or Occam's razor, because it's neither a well defined topic as are Popper and Kuhn who have put their philosophies on record, nor as it turned out, particularly well known among the scientists as is Occam's razor, or at least the simplicity principle. I therefore ended up writing in this chapter about the discourse about the term reductionism as it is being used in popular science, and discussing the philosophical concepts behind reductionism with the scientists in the interviews. These two ways of talking about the topic have not always been very comparable, though I will point towards some of the interesting comparisons that can be made, for example the very fact that while reductionism is a very hot topic in popular science, it received plenty of shrugs in the interviews.

Here I will present the way the popular science authors have written about reductionism, but I will particularly focus on a feature of the books that discussed reductionism that caught my eye. The topic was mentioned by some of the physics

authors, but of the life scientists almost exclusively by those who wrote on sociobiology or evolutionary psychology, or have been personally involved in the debates surrounding these fields. That reductionism is a matter of concern within the sociobiology/evolutionary psychology debate is well known and already well documented (see for example Ruse 1989, Segerstråle 2000, Ceccarelli 2001). I have therefore taken a slightly different route in this chapter, taking a detour in section 3.2 to look at two other samples of popular science books: first I survey books from the other side of the debate to recount how reductionism is talked about when people are negative towards evolutionary psychology and/or sociobiology, because unfortunately the Aventis prize shortlist has only included books on one side of the debate. Then I will present some quotes from throughout the career of one very central author in the controversy, E. O. Wilson, and chart how his talk about reductionism has changed over the years as he became involved in the debate.

I have also included the other books from the sample that talk about reductionism, all of them from the physical sciences, and all of them very positive in their evaluation (though different in the definition) of reductionism, which pretty much confirms the suspicion that as physicists have nothing much to lose from reductionism (as opposed to sociologists or even biologists), they can afford to be relaxed about it, or even use it as a weapon or identity marker against pseudo-science and postmodernism as for example Weinberg (1993) did during the science wars.

I will then present what the interviewed scientists said about reductionism. I have already hinted at some of the problems in comparing the responses to the popular science books because of a very low level of recognition of the concept of reductionism, which however is a very interesting result in its own right, and will be compared to the conclusion of the last chapter on Occam's razor, where I argued that there is a difference between the popular science and the "everyday" science discourse on Occam's razor, and this result therefore seems even more compelling in the case of reductionism. Because of these constraints I have concentrated on three things in that section, First I present the views of those scientists who have heard of reductionism and have been sure enough of themselves to venture an definition of it, which then can be compared to the books. I also present some of the

discussions I had about some of the philosophical issues that surround reductionism, which as in the previous chapters displays a wide range of views and some very interesting discussions. Finally, I will present some of the frequent views that I heard about how unimportant the problem is for practical science, which was a comment made so often in the early interviews that I started to ask the question myself in the later ones – the answer is an overwhelming agreement that while the issue is interesting as a philosophical problem, it is not practically important. This result is of course very interesting when compared to the passion with which it has traditionally been discussed in the popular science books and in the science wars.

This chapter will further develop my thoughts on the use of philosophy as an identity marker, which I think is particularly evident in the case of the sociobiology/evolutionary psychology debates, on which I will concentrate in my analysis. In this light, I think Gieryn's (1999, epilogue) analysis of the science wars as a boundary dispute is also helpful, since I have argued (in chapter 3) that Gieryn's approach and social identity theory are conceptually very close. I have already in the previous chapters pointed towards how I think some of the philosophies represented particularly in the popular science books can be interpreted as boundary or identity construction, but I believe that the social identity approach may be more fruitful in the final analysis of how reductionism gets interpreted by the authors involved in the sociobiology debates.

I will also tie these insights in with the more philosophical discussions I had with the interviewees regarding reductionism and more crucially, the philosophical issues behind reductionism. The contrast between the way reductionism and the issues behind it get written about and publicly contested in popular science, and the way they are discussed by non-involved scientists suggests some implications both for philosophers and scientists wanting to communicate science: Through these flexible interpretations that scientists have of reductionism in the sociobiology debates, the philosophical vocabulary can become fairly unusable to convey the scientists' actual opinions about the nature of science, which as the interviews show exist and are worth exploring. Scientific (and philosophical) discussion on reductionism, as well as effective communication of what science is, becomes impoverished as a result.

## 2. Philosophical introduction

Reductionism is frequently recognized to be a very contentious and ill-defined term (see for example Andersen 2001 for a review of the literature; Ruse 1994; and Dupré 1983). Given the amount of confusion on the topic that I found both in the popular science literature as well as the philosophical literature, I will start by giving a quick introductory definition of reductionism which reflects the way many people write about it.

A reductive explanation tries to explain “higher level” phenomena or theories (such as those of biology) solely by reference to “lower level” phenomena or theories (such as those of physics). This attempt at a quick introduction to the concept raises the immediate point that even invoking concepts such as “higher” or “lower” level sciences presupposes a hierarchy within the sciences that many non-reductionists would not necessarily accept. I will comment further on the apparent circularity here quickly in section 4.2 in response to a comment made by one of the interviewees.

One possible reason why some (non-physicist) scientists are uncomfortable with reductionism, is that it seemingly renders their discipline to be merely a branch of physics: Reductionism is very often seen to be threatening some sciences by reducing them to other sciences. In this context a frequent complaint about reductionism is that it reduces a science (or theory) to *nothing but* another science or theory. In this type of complaint an accusation is often made that reductionism disregards the complexity of the real world. The word “reductionist” is therefore often used synonymously with “simplistic”. The reductionist-as-simplistic usage of the term is very prominent especially in the social sciences, but also gets used by some biologists (see section 3.2 below). On the other side, reductionism is seen to be about the explanation of those complexities using simple or more fundamental premises, and therefore reductionism can be seen as a manifestation of Occam’s razor (Ruse 1989 p.58).

Philosophical introductions to reductionism often start with Nagel’s (1961) model (for example Curd and Cover 1998 ch.8). For Nagel a reduction is an explanation of a theory by showing that it can be logically derived from another theory. Before Nagel, reductionism was a concept that featured heavily in logical

positivism, but it carried slightly different meanings. One of these for example, the one criticized by Quine, was “the belief that each meaningful statement is equivalent to some logical construct upon terms which refer to immediate experience” (Quine 1980 [1953] p.20). Reductionism in the tradition of Nagel is often called inter-theoretic reductionism to distinguish it from the earlier approaches of the logical positivists.

What exactly reductionism is depends on what is to be reduced: in Nagel’s case it is theories that are “reduced” to other theories, in Quine’s sense it is theoretical statements that are “reduced” to observation statements. In general I will distinguish roughly between reductionisms that involve theoretical statements, such as models, theories, hypotheses or even whole disciplines, and those involving singular statements of fact or observation, such as phenomena or events. In most definitions of reductionism, theories and facts can be interchanged, but this is not always unproblematic. Thus we can say that both a theory or a fact can be explained by reference to another theory or fact, but the way this should work is not the same. Taking again Nagel’s reductionism, we cannot logically derive anything from a singular statement. Reductionism in the traditional logical positivist sense only makes sense when a theory is reduced to singular facts, but not other theories.

Secondly, the reason I put “reduced” into quotes above is that it is not clear what “reduce” should actually mean. For Nagel, it is quite clearly meant to be a type of explanation: We explain a fact or theory by showing that it follows logically from another theory. Other people can mean it to be more of an ontological statement: we are not concerned with whether the scientist actually manages to explain A by B, what matters is that it is theoretically possible; it is a statement about the structure of the world. These are of course also linked, because a belief that the world is structured in a reductive way would mean that a successful explanation should reflect that belief (this depends however on what we think a good explanation is, which is itself a contentious philosophical topic).

Thirdly, for each way in which we can define reductionism, there can be two kinds of people who actually call themselves reductionists: We can either claim that science always strives for reductionism, or that it is merely desirable (or that reality is only ever structured in a reductionist way, or just mostly). I believe that this particular point is where most of the misunderstandings arise from. People who

argue for reductionism generally argue that reductionism is merely desirable, while people who argue against reductionism often claim that reductionism requires science always to be reductionist. For convenience I will call these two options weak and strong reductionism below. The opposite of reductionism is traditionally said to be holism, although I will be trying to limit my use of this concept, as its meaning is as much debatable as that of reductionism, if not more. While most anti-reductionists claim to be holists, and vice versa, there are popular science authors who argue against both (for example Deutsch 1997 p.21).

### **3. Reductionism in the popular science books**

Eight of the books mention reductionism or reduction and offer an explanation of what it is. These explanations will be shown below. Of the other books several mention reductionism (or holism) but leave the terms unexplained. Again it has to be noted, as with the other topics discussed above, there are disproportionally many physical science books represented, and that I think this shows an actual divide between fundamental versus theoretical sciences in their attitude towards philosophical topics.

In this particular case, however, the books within the life sciences that mention reductionism reveal a rather interesting pattern, which relates to the author's affiliation in the Nature/Nurture debates, and will be discussed further below. To emphasise this, I have presented the quotes below divided into life sciences and physical sciences. I will first concentrate on the life sciences books and recount the particular (popular) scientific controversy they almost all represent. Because of the nature of my sampling, there were no the popular science books from the "nurture" side of the debate in the sample, and therefore I will present some examples of what the opposition has written about reductionism, again in a popular science book setting. Finally, I will look at the historical development of the talk about reductionism throughout the career of a writer who is very central to the controversy, E. O. Wilson, and chart how his use of the word reductionism has changed as he got embroiled in the debate. In the concluding section to this chapter, I will then expand on how I see all this fits into a social identity theory framework (see chapter 3), and with the other philosophical topics discussed in this chapter.

### 3.1. *The life sciences*

The zoologist Matt Ridley (Ridley 2003) considers himself a reductionist. At the same time, he argues that this position is frequently criticized. His argument for reductionism uses a rather sarcastic rhetoric:

Even to ask such a question reveals me to be a reductionist, and reductionists are BAD THINGS. We are supposed to glory in the holistic experience, and not try to take it apart. (Ridley 2003 p.163, original emphasis)

This passage shows that Ridley expects the reader to have at least some familiarity with the term already. In fact, it is not until much later in the book that he comes closer to an explanation of what reductionism is, or rather an explanation of what definitely is *not* reductionism. Here Ridley is setting himself up in a conscious opposition, arguing against an undefined mob that forces us to “glory in the holistic experience”. He implies of course that this received view is incorrect. He uses a rhetoric here which is similar to the “silent majority” argument, by implying that just because we are “supposed to” by the fashions of the day, does not mean it is good scientific practice.

A further explanation of what reductionism actually means comes only a long time after that passage. In discussing the sociology of Durkheim, he remarks that

[Durkheim said:] ‘The determining cause of a social fact should be sought among the social facts preceding it and not among the states of individual consciousness.’ In other words, he rejected all reductionism. (Ridley 2003 p.247)

This implies that reductionism means that we should *always* look at the states of individual consciousness to have caused a social fact (Because Durkheim rejected *all* reductionism: If some version of reductionism allowed us to seek another type of cause when appropriate, Durkheim could have accepted it). This then seems to correspond to what I called “strong reductionism” above. Also, in terms of my classification of reductionism, Matt Ridley seems to be talking about a reduction



being about facts and explanations. This very strict interpretation is probably not completely fair to Ridley, because this definition of reductionism defines it only by what it is not. In other places he seems to suggest that reductionism is not always appropriate, for example in his book "Genome" he qualifies earlier chapters by admitting that he has "fallen into the habit of reductionism" (Ridley 1999 p.148).

E. O. Wilson, while presently also considering himself a reductionist, talks differently both about the definition of reductionism and its role in science: "The cutting edge of science is reductionism, the breaking apart of nature into its natural constituents" (Wilson 1998 p.58). This definition of reductionism represents a subtly different form than the one (apparently) advocated by Matt Ridley. It is enough to reduce as far as naturally possible, but gives us no injunction to look further than that: it is perfectly possible even for a social fact not to have any more natural constituents. In fact, his assertion that reductionism represents (merely) the leading edge of science, suggests that there are other legitimate, if less successful parts of science that have nothing to do with reductionism. It is therefore a version of weak reductionism, both in the methodological and the ontological sense.

Wilson's initial introduction to reductionism makes his subsequent explanation of how reductionism works somewhat perplexing, because he seemingly equates reductionism with what he perceives as good scientific practice. In a way, this is a rather telling point about Wilson's attitude towards non-reductionists: they do not even do proper science.

Here is how reductionism works most of the time, as it might appear in a user's manual. Let your mind travel around the system. Pose an interesting question about it. Break the question down and visualize the elements and questions it implies. Think out alternative conceivable answers. Phrase them so that a reasonable amount of evidence makes a clear-cut choice possible. If too many conceptual difficulties are encountered, back off. Search for another question. When you finally hit a soft spot, search for the model system-say a controlled emission in particle physics or a fast-breeding organism in genetics – on which decisive experiments can be most easily conducted. Become thoroughly familiar – no, better, become obsessed –

with the system. Love the details, the feel of all of them, for their own sake. Design the experiment so that no matter what the result, the answer to the question will be convincing. Use the result to press on to new questions, new systems. Depending on how far others have already gone in this sequence (and always keep in mind, you must give them complete credit), you may enter it at any point along the way. (Wilson 1998 p.58, apart from the first sentence, all of the original quote was in italics)

Even more confusingly, Wilson proceeds by explaining a viewpoint that he confesses to agreeing with, which is a strong version of reductionism because it proposes that *every* law can be reduced. This viewpoint, which he calls “total consilience”,

holds that nature is organized by simple universal laws of physics to which all other laws and principles can eventually be reduced. This transcendental world view is the light and way for many scientific materialists (I admit to being among them), but it could be wrong. At the least, it is surely an oversimplification. At each level of organization [...] phenomena exist that require new laws and principles, which still cannot be predicted from those at more general levels. (Wilson 1998 p.59)

Wilson’s attitude towards reductionism therefore seems more complex than he suggests at first. While a weak form of reductionism is so uncontroversial that it is almost the same as scientific method itself, the stronger form of reductionism, which he subscribes to, is recognized as controversial. But actually, even though he admits that he subscribes to the strong version, he immediately qualifies this by saying it is an oversimplification.

Ceccarelli (2001 p.142) has also remarked on Wilson’s equation of reductionism with scientific method. She shows how Wilson keeps changing between strong and weak reductionism throughout his book. She calls this rhetorical strategy “polysemous textual construction”, which she identifies as “a passage that can be read (that is interpreted) in two or more ways” (2001 p.5). In this way the author can appeal to different audiences, though in the case of Wilson,

Ceccarelli argues that the strategy was unsuccessful and that Wilson was “uniformly” interpreted as holding a strong reductionist position (2001 p.139).

Wilson’s concept of “consilience” has often been interpreted as being a version of reductionism itself, though Wilson introduces it as an alternative word for “coherence” between the sciences (Wilson 1998 p.6). Also, Wilson’s earlier work, the notorious *Sociobiology* (Wilson 1975) itself, has been often seen as a reductionist account due to its aim to connect biology to the social sciences in a similar way to how it is done in *Consilience* (see also Lyne and Howe 1990). As I will show below, while Wilson’s motivations might have been the same in the two books, in *Sociobiology* the concept of reductionism is not explicitly identified with consilience or coherence between the sciences. On the contrary, in the earlier book Wilson appears to argue *against* reductionism.

Steve Pinker also labels himself as a reductionist although, like Wilson, he argues that there are two forms of reductionism: “Reductionism, like cholesterol, comes in good and bad forms” (Pinker 2002 p.69). Of the bad reductionism, Pinker argues that it is not in fact a straw-man, because some scientists have actually held this point of view. Bad reductionism “consists of trying to explain a phenomenon in terms of its smallest or simplest constituents” (Pinker 2002 p.70). That Pinker thinks this is such a misrepresentation of reductionism that it could be misperceived as a straw-man is surprising, because, on the face of it, it is equivalent to Wilson’s weaker (and supposedly uncontroversial) reductionism. Pinker, of course, sees himself as a good reductionist: Good reductionism “[...] consists not of replacing one field of knowledge with another but of connecting or unifying them” (Pinker 2002 p.70). In the context of his explanation, this seems to mean that Pinker merely requires the different sciences to be complementary and not contradict themselves. This version of reductionism is so watered down that it is hard to imagine how any serious person can disagree with it. Pinker thus almost manages to build up an *anti*-straw-man: this explains Pinker’s incredulity that anyone can fail to be a reductionist. But, in fact, we can easily hold an opinion which is similar to Pinker’s (i.e. one of the unity of the sciences) and be resolutely anti-reductionist, as long as under reductionism we understand something like Wilson’s strong or even weak versions of reductionism. Note also that bad reductionism is about the reduction of

phenomena, while good reductionism is about “fields of knowledge”, or scientific disciplines.

A similar view of science to the one advocated by Pinker is found in Ernst Mayr (1997), in a generally very philosophically orientated book, although Mayr actually arrives at an anti-reductionist conclusion. Mayr stayed more or less neutral in the sociobiology controversies (see Shermer and Sulloway, 2004), but explicitly argues against reductionism. Elsewhere (see Ruse 1996 p.445) he explains that it was his desire to refute reductionism that drove him to write philosophical works in the first place. (Ruse also relates that this desire came from Mayr’s opposition to molecular biology; see the discussion on the early Wilson below.) Arguing for the unity of science, he nonetheless cautions against reductionism:

[A]n advocate of the autonomy of biology might argue in the following way: Many attributes of living organisms that interest biologists cannot be reduced to physicochemical laws, and, moreover, many aspects of the physical world studied by physicists are not relevant to the study of life (or to any other science outside of physics). [...] A unity of science cannot be achieved until it is accepted that science contains a number of separate provinces, one of which is physics, another of which is biology. It would be futile to try to "reduce" biology, one provincial science, to physics, another provincial science, or vice versa. (Mayr 1997 p.32)

Mayr’s opinion of how science actually works or should work is almost the same as Pinker’s good reductionism. Moreover, it is even compatible with Wilson’s weak reductionism because Wilson never disputed that some things are not reducible, even though Mayr puts more emphasis on this argument, while Wilson plays it down. It is certainly compatible with what Pinker almost calls a straw-man, bad reductionism, because Mayr does not forbid scientists from “[explaining] a phenomenon in terms of its smallest or simplest constituents”.

Finally Mark Ridley (2000) makes a rather curious comment on reductionism, calling it an “ersatz language”, which can either signify that he means it to be a fake

language, or, in the more original German sense of the word, a replacement language. Mark Ridley has not directly contributed to the popular science book debate on Nature vs. Nurture, though like Mayr's book, his book covers several themes that are also covered in some of the books I considered above.

I can make the same point in the ersatz philosophical language of reductionism. We have three kinds of theory. [...] In most non-Darwinian explanations of complexity, the first kind of theory is enough: the mutations alone drive the evolution of complexity. The evolution of complexity can be reduced to the mutation process. In the Darwinian explanation of complexity, however, the third kind of theory does not reduce to the second, and the second does not reduce to the first. We have to work out all three on their own merits. (Ridley 2000 p.25)

The fact that he argues against reductionism in this instance suggests that he is less enthusiastic about reductionism, though unfortunately in this book he does not give either a definition of what he means by reductionism, or a statement on whether he considers himself a reductionist.

### *3.2. Situating reductionism within the Nature/Nurture debate*

All the authors in the life sciences books have in fact contributed to a particularly bitter debate that has been fought out within popular science, and therefore deserve to be analysed somewhat separately from the others. In this section (3.2), I will briefly give the background to the debate, show some contrasting quotes from other authors engaged in it, and lastly chart the development of the concept of reductionism in one central figure within the debate, E. O. Wilson. In section 5.1, I will then rejoin the discussion in light of what the other authors have written on it, how they wrote about the other subjects discussed in this chapter, and how I intent to make sense of it, focusing mainly on social identity theory and theories on boundary work discussed in chapter 3.

The sociobiology and evolutionary psychology disputes arise out of a longstanding debate about how much of human behavior is caused by our nature (or

our genes), and how much is caused by our upbringing. One particular semi-popular book which attracted a lot of attention was E.O. Wilson's *Sociobiology: the new synthesis* (Wilson 1975), which has been severely criticized by, among many others, Wilson's Harvard colleagues Richard Lewontin and Stephen J. Gould. Wilson waded into this area while in the background at the time there were a number of controversies surrounding the issues of IQ research and eugenics, and so the terms in which these controversies were debated were also very quickly applied to sociobiology. Because sociobiology aimed at explaining the social behavior of humans through evolutionary mechanisms, it was accused of being an excuse for eugenicists and racists to claim their views as scientifically based. In this earlier debate, reductionism was already one of the contentious terms, as the IQ researchers were accused of reducing humans to nothing but their genes. At about the same time, Richard Dawkins published his semi-popular book *The Selfish Gene* (Dawkins 2006 [1976]) which also found itself embroiled in the controversy.

In the 1990s a second wave of writers emerged who distanced themselves somewhat from sociobiology itself, but still saw their work as part of the tradition started by Wilson and Dawkins, calling their approach "evolutionary psychology". These include authors like Steve Pinker and Matt Ridley who together with Wilson himself also featured in my sample, and who have been debating the same opponents, as in the earlier round. Segerstråle 2000 gives an extensive overview of the development of the debates and their origin. Cassidy 2005 and 2006 gives an interesting analysis of the later, evolutionary psychology stage of the debate from a science communication point of view.

Settling on a name and a quick summary of the dispute itself is somewhat difficult because the fields that concern themselves with the evolutionary study of human behavior have undergone several name-changes and changes in emphasis. It is probably precisely because this range of subjects has been so controversial that any new developments are dressed up as new disciplines to distance it from its predecessors (see Segerstråle, 2000 p.317). In this chapter I have made the decision to refer to the debate as Nature/Nurture. Even though in many ways these terms simplify the dispute itself, I have found talking about being "pro or anti sociobiology or evolutionary psychology" too clumsy a phrase. Referring to Nature/Nurture may actually have the advantage of putting the debate into the

historical context in which reductionism has been so contentious, and reminds us that the controversy did not simply start with the publication of *Sociobiology* and *The Selfish Gene*.

The Nature/Nurture dispute is also unusual for a scientific dispute because it has also been often fought out within popular science media rather than just the academic circuit. This is one reason why this particular debate may be interesting to look at from a science communication point of view, because here the reader can even have a sense of participating directly by reading some of the primary sources themselves. Many of the classic works cited in the debate, such as Dawkins 2006 [1976] and Wilson 1975 are popular or semi-popular books themselves and therefore very much accessible to the interested layperson. Also the way ideas are discussed in these books is sometimes slightly different to “regular” popular science. Rather than being an authoritative account of what we know, as popular science can often appear, the books in this debate are often trying to persuade and argue for a particular viewpoint and appear to be often directed at each other as much as at the public. That emphasis on persuasion which is removed from the stylistic constraints of the technical literature may also be a reason why philosophical topics such as reductionism feature more prominently here than in other popular science subjects. These exchanges have of course not exclusively been conducted in the popular science sphere, but still very visibly so (Cassidy 2005, 2006), and certainly much more than is usual in science.

In all of these disputes, the authors can very visibly be divided in two groups, even if only through their opposition to or support of Wilson’s earlier work. On one side are popular scientists like Wilson and Dawkins who are joined by a group of popular science writers on evolutionary psychology. On the other side stand Wilson’s and Dawkins’ principal opponents Richard Lewontin, Stephen J. Gould and Steven Rose, who are however not joined by an influential younger generation of popular science authors.

The camps are also visibly divided by their support of, and opposition to, reductionism, as many observers and even some protagonists have pointed out (Segerstråle 2000 ch.14; Ruse 1989; Pinker 2002). In this chapter I do not intend to add another study that points to this result, although I believe a reading of my sample corroborates it. Instead as mentioned earlier I will look at the underlying

philosophical message behind the various representations of reductionism, and thereby go beyond just identifying who calls himself a reductionist and who does not.

In my sample, only authors who contributed to the “Nature” (or sociobiology or evolutionary psychology) side of the debate were represented.<sup>33</sup> Of these, all of them mentioned reductionism. In fact the only two life science authors in my sample who mentioned reductionism who did not visibly take sides in the dispute, Ernst Mayr and Mark Ridley, were involved in other ways: Mayr has frequently commented on the dispute but stayed neutral (see Segerstråle 2000 and Shermer and Sulloway 2004). Mark Ridley was a former student of Dawkins (mentioned on the cover of Grafen and Ridley 2006), and therefore unlikely to be neutral, though he himself has not contributed to the debate.

Because I found that there is such a strong correlation between arguing for sociobiology/evolutionary psychology and arguing for reductionism, I will briefly introduce how the principal opponents of sociobiology talked about reductionism. I will then show some quotes on reductionism taken from earlier work by E. O. Wilson before and during he became embroiled in the sociobiology controversy, and chart his changing attitude towards it.

#### *Reductionism in “pro-nurture” books*

Among the most prolific popular science authors who argued against sociobiology is Stephen J. Gould, who also consistently argues against reductionism, devoting a whole chapter to arguing against it (and against Wilson’s concept of consilience) in one book (Gould 2003 pp.198-260). In an earlier book, when discussing Dawkins, he remarks:

I think, in short, that the fascination generated by Dawkins’s theory arises from some bad habits of Western scientific thought – from attitudes [...] we call atomism, reductionism, and determinism. The idea that wholes should

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<sup>33</sup> None of the authors from the “Nurture” side were actually shortlisted for the Aventis/Rhône-Poulenc prize in the period I considered. I do not think this represents a systematic bias, because the



be understood by decomposition into “basic” units; *that properties of microscopic units can generate and explain the behavior of macroscopic results*; that all objects have definite, predictable, determined causes. (Gould 1980 p.77, my emphasis)

Here, Gould associates Dawkins’ theory not merely with reductionism, but also with atomism and determinism. While his description of atomism can be seen as a version of ontological reductionism, determinism and reductionism are independent philosophical viewpoints which are very often associated together by writers opposed to both of them (materialism and mechanism can be added to this list as well). It could be argued that this association is predisposing people who wish to argue against determinism, against reductionism, so that the author ends up believing reductionism to be whichever version accords closest to his/her views on determinism. Reductionism here is taken to mean the belief that microscopic units can “generate and explain the behavior of macroscopic results”. In other words, Gould seemingly believes this can never be the case.

However, in other works a slightly more relaxed anti-reductionism is favoured:

The depth [of determinism] records the link of biological determinism to some of the oldest issues and errors of our philosophical traditions – including *reductionism*, or the desire to explain partly random, largescale, and irreducibly complex phenomena by deterministic behavior of smallest constituent parts (physical objects by atoms in motion, mental functioning by inherited amount of central stuff). (Gould 1992 p.27 original emphasis)

Here there is no suggestion that it is not at least sometimes permissible to explain reductively, because we are not talking of *any* macroscopic results, but only irreducibly complex ones. The implications are that Gould is in fact allowing the occasional reductive explanation, where appropriate. Note again Gould’s association of determinism and reductionism.

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“Nature” side simply seems to have published more popular science books recently. Gould did in fact win the Rhone-Poulenc prize in 1991, while Rose won in 1993.

Steven Rose shows his anti-reductionist credentials by remarking that: "...I have spent a considerable portion of my theoretical energies over the years criticizing reductionism" (Rose 1992 p.210). As an explanation of what reductionism means he remarks that it possesses an "insistence that in 'the last analysis' the world can be explained in terms of atomic/quantum properties and a few universal assumptions" (Rose 1992 p.74). This would make Rose's version of reductionism an even stronger one than Pinker's bad reductionism, i.e. that *everything* ultimately is explainable by fundamental physics – and therefore something that Pinker (or even Wilson) might consider an unfair straw-man representation of reductionism.

Situating this description of reductionism within a discussion of Descartes' materialist philosophy, Rose consistently associates reductionism with mechanism: "...I could only be amazed at how deeply my thinking had become trapped into a mechanistically reductionist straightjacket" (Rose 1992 p.287). Interestingly, Rose distinguishes between methodological and philosophical reductionism, with only the latter being the subject of his criticism. Methodological reductionism is to "try to stabilize the world that one is studying by manipulating one constant at a time, holding everything else as constant as possible" (Rose 1992 p.210). I do not, however, see any connection between Rose's "methodological reductionism" and any of the forms of reductionism I have been discussing so far. Nor does it seem to prevent Rose from consistently describing himself as being against reductionism. It is interesting then that Rose, just like Wilson and Pinker, identifies a form of reductionism he agrees with, and one that he does not agree with – but unlike them he does not think that would make him a reductionist.

Finally, Richard Lewontin also argues against reductionism, by which he means

[t]he belief that the world is broken up into tiny bits and pieces, each of which has its own properties and which combine together to make larger things. (Lewontin 1993 p.107)

Here Lewontin is talking about an ontological position rather than explanation or a methodological prescription of how to do science. Reductionism as a methodology is understood to be of the very strong kind, i.e. that every explanation has to be reductive.

Now it is believed that the whole is understood *only* by taking it into pieces, that the individual bits and pieces, the atoms, molecules, cells, and genes, are the causes of the properties of the whole objects and must be separately studied if we are to understand complex nature. (Lewontin 1993 p.12 original emphasis)

Just like Rose, Lewontin associates reductionism with mechanism; he writes for example about “[t]he difficulties of the reductionist mechanical view of biology...” (Lewontin 2000 p.45). Ironically, it was Lewontin (writing together with Richard Levins) who, 15 years earlier, expressed impatience with the constant confusion of reductionism and materialism (Levins and Lewontin 1985 p.133)

#### *The development of reductionism in the works of E. O. Wilson*

As has been remarked by other observers of the sociobiology dispute, some actors may not be as reductionist or anti-reductionist as they claim. Segerstråle for example quotes Maynard Smith as saying that “Dick Lewontin is an old-fashioned mechanistic reductionist” (Segerstråle 2000 p.288). Similarly, Wilson has been described as “holistically orientated” (Segerstråle 2000 p.290). In fact, Wilson has undergone quite a change in his talk about reductionism, and this otherwise puzzling transformation can possibly be explained by the different groups that Wilson has been battling and belonging to over the years. Before the publication of *Sociobiology*, for example, there had been a real fear among zoologists that the evidently “reductionist” discipline of molecular biology<sup>34</sup> was going to replace mainstream biology (see his autobiography, Wilson 1994 ch.12). So, reductionism

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<sup>34</sup> I am describing molecular biology as “evidently” reductionist, because it attempts to explain biological phenomena in terms of molecular ones. Where in my own classification of reductionism it stands depends on whether molecular biologists want to explain everything like that. Since the fear expressed by Wilson, Mayr and other zoologists and evolutionists was that they would be made redundant by molecular biologists, they presumably saw it as a form of “strong reductionism”.

may not always have been something positive for Wilson. This is reflected in an early textbook on insect societies where it appears only as an adjective to theorists he disagreed with:

Albrecht Bethe, an extreme reductionist, believed that ants are 'reflex machines' [...]. [Theodore C. Sheirla's] intent, I believe, was also reductionist [...]. It is now very clear that neither of these opposing simplistic schemes accurately identified the innate and experiential elements of behavior. (Wilson 1971 p.221)

Note Wilson's association of reductionism with simplistic explanation. This of course also ties in to the discussion on Occam's razor above, and as mentioned in section 2, the concepts of simplicity and reductionism have a lot in common.

Later, in the beginning of his famous book "Sociobiology", he talks about a "new holism", how it stands in direct contrast to reductionism, and how his study is meant to be holistic. I think there are quite a few things that stand out in this quote, so I have numbered different passages for ease of analysis.

[1] The recognition and study of emergent properties is holism, once a burning subject for philosophical discussions [...], [2] but later, in the 1940's and 1950's, temporarily eclipsed by the triumphant reductionism of molecular biology. [3] The new holism is more quantitative in nature, supplanting the unaided intuition of the old, it does not stop at philosophical retrospection but states assumptions explicitly and extends them in mathematical models that can be used to test their validity. [4] In the sections to follow, we will examine several properties that are emergent and hence deserving of a special language and treatment. (Wilson 1975 p.7)

First of all, passages one and four together show that Wilson means his work to be holistic, because his book will deal with emergent properties, the study of which he says is holism. In passage two he identifies reductionism as a conceptual rival to holism, but one which has had its day. Most interesting I find his description

of “new” holism (passage three) which, just like his later description of reductionism, actually seems to be his idea of good scientific method. This description of holism as studying emergent phenomena is, remarkably, very much compatible with the position he will later identify with “weak” reductionism and (but this depends on his treatment of emergent phenomena), even possibly “strong” reductionism. Finally, (in passage two) he also reveals that his criticism of reductionism is directed at molecular biology.

Only three years after the publication of this book, with the row it engendered now in full swing, he starts talking differently about reductionism. Though he still thinks there is more to science than “raw” reductionism, he no longer places it in opposition to his own holism, but sees it rather as complementary. He starts by approving of Mach’s reductionist philosophy, and then adding on to it:

[According to Mach] the heart of the scientific method is the reduction of perceived phenomena to fundamental, testable, principles. [...] Although Mach’s perception has an undeniable charm, raw reduction is only half of the scientific process. The remainder consists of the reconstruction of complexity by an expanding synthesis under the control of laws newly demonstrated by analysis. (Wilson 1978 p.11)

He ends this passage by lamenting that “[r]eduction is the traditional instrument of scientific analysis, but it is feared and resented.” (Wilson 1978 p.13) Wilson here seems to have philosophically been fighting two enemies. First, against molecular biology, which is traditionally described as reductionist (because it explains biological phenomena by what happens at the molecular level), he identifies as a holist. When, in the sociobiology debates, he gets accused of reductionism, he becomes a reductionist. Note that I am not accusing Wilson of inconsistency here, as he is perfectly entitled to change his opinion. In fact, Wilson has noted (1994 ch.12) that over the years he came to see many of the molecular biologists’ points, and reductionism seems to be one of them.

But now Wilson talks as if he had always been a reductionist. Writing about the 1960s, he states that “I believed deeply in the power of reductionism, followed

by a reconstitution of detail by synthesis” (Wilson 1994 p.255). This suggests that Wilson must think that in the early 70’s he was only wrong about his *definitions* of holism and reductionism, rather than about the content of his opinion on how science works.

### 3.3. *The physical sciences*

The physicist Mark Buchanan quotes the dictionary definition of reductionism:

Reductionism, according to The Concise Oxford Dictionary, is defined as “analysis of complex things into simple constituents; the view that a system can be fully understood in terms of its isolated parts, or an idea in terms of simple concepts.” [...] Reductionism is the idea that the best way to understand any complicated thing is to investigate the nature and workings of each of its parts. This approach is how we solve problems, and it comprises the very basis of science. (Buchanan 2002 p.198)

This sense of reductionism does not involve talk of theories reducing to more fundamental theories, nor does it mention disciplinary unification in the manner of Pinker’s good or hierarchical reductionism. It does, however, mention that reduction can occur on the level of “things” and on the level of ideas or concepts.

Although Buchanan asserts that reductionism “comprises the very basis of science”, he also cautions that there is more to science than that. From the start of the book he signals that he dislikes the conventional reductionism of the dictionary type.

For centuries scientists have been taking nature apart and analyzing its pieces in ever-increasing detail. By now it is hardly necessary to point out that this process of “reduction” can take understanding only so far (Buchanan 2002 p.6)

However, reductionism can be saved by getting a better definition which would

assert that a system can be fully understood in terms of its parts *and the interactions between them*. And yet it is also important to acknowledge that the interactions between the parts of a complex network often lead to global patterns of organization that cannot be traced to the particular parts. (Buchanan 2002 p.199, original emphasis)

Like Pinker, Buchanan notices that reductionism can mean different things, although his distinction between the “good” and the “bad” meaning of reductionism is a different one.

In his very philosophical book, Deutsch helpfully provided a glossary at the end of each chapter and where he defined reductionism like this: “reductionism: The view that scientific explanations are inherently reductive. (Deutsch 1997 p.30), where “reductive” means: “reductive: A reductive explanation is one that works by analysing things into lower-level components (Deutsch 1997 p.30)”.

Deutsch talks about reduction being a form of scientific explanation, much more in the Nagelian sense. His dissatisfaction with reductionism is that it does not account for “higher-level” phenomena that cannot be explained by, say, particle physics, such as the laws of evolution. This point is essentially that made by Mark Ridley and seems to be one of the main criticism people level at (Nagelian) reductionism. But notice that Pinker’s “good” reductionism is entirely compatible with that point. And so is even the (non-Nagelian) weak reductionism of Wilson, who wants to “break nature apart into its natural constituents”, because if a higher-level phenomenon, like a fact of biology or sociology, just has not got any more natural constituents, then it is reduced as far as possible.

Brian Greene however does not think that higher level laws represent “new and independent laws of physics” (Greene 2000 p.17). Greene’s reductionism involves the idea that everything can be explained on the basis of fundamental laws of physics and that the incalculable complexity of higher level laws (such as those concerning tornados) are only a matter of “calculational impasse”, i.e. human limitations. He also acknowledges that his is a contentious point and identifies his view as “staunch reductionism”, which he describes thus:

A staunch reductionist would claim that [...] in principle absolutely everything, from the big bang to daydreams, can be described in terms of underlying microscopic physical processes involving the fundamental constituents of matter. If you understand everything about the ingredients, the reductionist argues, you understand everything. (Greene 2000 p.16)

But Greene is also being careful. Right after his description and defense of “staunch reductionism”, he writes:

What is largely beyond question, and is of primary importance to the journey described in this book, is that even if one accepts the debatable reasoning of the staunch reductionist, principle is one thing and practice quite another. (Greene 2000 p.16)

Greene actually talks of a “spectrum of reductionism”, and it is clear that he thinks his major points are still valid if you do not agree with him on “staunch” reductionism.

#### **4. Reductionism in the interviews**

The concept of reductionism in science is in one important way different to that of simplicity, which is that simplicity is a word that is instantly recognised and appreciated. By contrast I found that reductionism is not a very widespread term within science. In this respect the extensive discussions on reductionism in the popular science forum discussed above are strange, because it is not only frequently supposed to be a term which is familiar with scientists, but also that it is seen as important enough to talk about to the public. Here we may have to disentangle the use of the term as an identity issue which is probably aimed more at other scientists rather than the public, as I will argue below, and what the same people think about the nature of science in respect to reductionism, or concepts like it.

In this section I will contrast the popular science concepts of reductionism to the way the scientists in the interviews have responded to it. Here I faced a particular difficulty. In chapter 5 on Popper and Kuhn, the philosophers themselves



and their philosophical terms may have been relatively obscure, but the terms in which they are discussed are clear (maybe a little bit less so for the case of Kuhn). On the topic of simplicity, while it turned out the principle of Occam's razor itself is philosophically contentious, at least everybody could relate to it because simplicity is an everyday concept.

In the case of reductionism neither is quite the case: many people had not heard of reductionism, and in any case, as shown in the previous sections, reductionism has no clear agreed philosophical meaning either. Therefore in this section I will first present the definitions of reductionism that I received from the small number of scientists who knew of the term and were confident enough to give me their own interpretation of it. The main part however will be occupied by *my own* description of reductionism, which will not necessarily correspond to some of the ways in which it has been discussed in the books, though I have tried to distill what I think are the philosophically most interesting questions surrounding reductionism, the existence of fundamental laws in the "higher"-level sciences, and following on from that, the nature of explanation.

#### *4.1. Scientists' own definitions of reductionism*

Of the twenty scientists who recognised the term reductionism, only 12 were confident enough to venture their own definitions, which in some cases were quite short and pithy, in others very hesitant, in others very opinionated and forceful, and in others resting on a misunderstanding. In this subsection I will quote most of these, which shows quite colourfully the really quite different ways the term is understood, and contrasts very much to the way the concept is fought over in the popular science books that I have quoted.

First, just like the popular science authors above, some scientists have not only heard about the concept, but thought about it themselves and come to a conclusion. That was usually with those scientists who have confessed to having some considerable interest in the philosophy of science, for example this respondent, whom I have already quoted in chapters 5 and 6 as a firm supporter of Popper and Occam's razor. Although his idea of reductionism is fairly unrelated to the concept as used in philosophy, what is interesting here is that he identifies reductionism with the set up of an experimental design, echoing the "methodological reductionism"

which I quoted Steven Rose above as agreeing with, but he immediately qualifies this by remarking that this is more of an idealization and not what really happens in science:

I think one would say that there's a typical experimental design is a reductionist design, which is to vary one variable at once. In terms of how you manipulate entities. But we all accept that that's not how reality is, and that in biology no process happens independently of other processes. (7 Senior, Biology, Male, UK)

The focus on experiments for reductionism was made again by another scientist, for whom reductionism meant "reducing experimental things to its simplest tool. Making experiments less complicated." (11 Mid-Career, Biology, Male, UK). It has to be noted though that this respondent was much less sure of himself that he got the meaning of reductionism right, qualifying this remark by adding "I assume that's what you're talking about." When I asked whether reductionism means much to him, he replied that it did not really.

Also voiced was the idea that reductionism means everything can be explained by mathematical or logical laws, here there may be a connection to the ontological razor discussed in chapter 6, but it does not assume the fundamentality of any particular discipline as such. However, this scientist goes on to explain what he means by his reductionism through "Laplace's demon", a famous philosophical argument which tries to show that if we know the state of every particle we can predict the future. Here we then also have the connection between reductionism and determinism.

what reductionism, to me, means, is that there are large parts of science which you can formulate laws, normally in the form of mathematical equations, possible logical equations, but normally mathematical equations. These mathematical equations can be used to predict, they're very often first order differential equations, there may be difference equations, things like this. They can be integrated, and they really stem, I suppose from the ideas of Newton. And the sort of Laplacian idea, it was Laplace, wasn't it, who

said that if they knew the position of every particle, you could predict the future, and so on... (6 Senior, Chemistry, Male, UK)

This person was very positive towards reductionism as he understood it, arguing that he is “actually a believer in trying to find as much reductionism in the system as possible.” This incidentally does not translate into the “strong” reductionism which aims for reductionism for everything, rather than only for as often as possible. This is even admitted by this scientist, who also argues that he is interested in “emergent phenomena”, and that there are things, like entropy or evolution, which cannot be explained in terms of the behavior of individual molecules.

Emergent phenomena have also featured centrally in other definitions of reductionism, as has the idea that reductionism represents a “hierarchy”.

I mean I have a hierarchy, and I think most biologists do, have a hierarchical view about the structure of life, and that there are emergent phenomena. And that there're things happening at higher levels of biological organization that are not... wholly predictable from the mechanisms that we understand at a lower level. So for example you cannot use quantum physics, well apparently, you cannot use quantum physics to explain the behavior of water molecules, so you certainly can't use quantum physics to explain the behavior of that great tit [points out of the window]. (12 Senior, Biology, Male, UK)

That you cannot use quantum mechanics to predict the behaviour of a bird can be seen as an argument against reductionism, and it is therefore interesting that this particular scientist then goes on to say that he is not sure that there are too many emergent phenomena in science, and that he therefore sees himself as a reductionist. He even went on to tell me about heated discussions he had with a colleague of his who sees himself as a definite anti-reductionist, who he thinks uses reductionism himself without quite realizing.

I'm not sure how many emergent phenomena there are in ecology, I mean it's a good question, I mean, to some extent it's a get-out, you know, you

have to be careful of, if you claim something is emergent, then you give up looking for explanations at a lower level. And they might in fact be there, you know, so I'm a bit suspicious of emergent phenomena. But, and therefore a reductionist [laughs]. But you have to be, I mean even people who argue against reductionism as a philosophy tend to use it in their science. (12 Senior, Biology, Male, UK)

Comparing this response to reductionism with the popular science discussion of reductionism shown above, this comment seems similar to the ones where the identification with reductionism is made strongly, even if only on the basis that we use reductionism only some of the time, rather than all of the time. This strong identification with a particular view of reductionism was actually quite rare in the interviews (see below on my question on the importance of reductionism).

Another scientist saw reductionism as “constantly” going down to the molecular level, and only working there. Note also that on responding to my question this scientist immediately saw reductionism as the opposite to holism. Here I asked him what he understood by reductionism.

Versus holistic approaches? Like my experience in the past is the question of whether or not one should just go *really* down you know, and become incredibly focused on a small range of questions which might be molecular in nature, and that would mean for me as being reductionist, it is constantly going down, and only working at that molecular level. (13 Mid Career, Biology, Male, UK)

This was that particular respondent's initial reaction towards reductionism; however, in the following discussion he told me that he used to have a much stronger reaction against reductionism in the past, and that nowadays he concedes that reductionism has its uses. This admission of some worth for reductionism shifts his conceptualization of reductionism more into the “weak” sense, where reductionist approaches exist alongside holistic ones.

I was really dead against reductionism, because it was at the age when molecular stuff was taking off, and it just bored the hell out of me, but now I'm more probably accepting of the fact that a) that work has to take place, so yes, somebody has to be a reductionist, at least in part of the work they do, but only, it will only really succeed if that reductionist approach is then scaled up, you know, into a more holistic sort of context. (13 Mid Career, Biology, Male, UK)

Another point to note is that both these last two quoted scientists are not only both working in biology departments, but both describe themselves as working in plant ecology, which shows that quite different pictures of reductionism can be found within the same relatively narrow subfield.

Finally there was one scientist who explicitly made the connection between reductionism and materialism that was talked about so often in the books that were against reductionism. At least as applied to materialism, he also argued that there is a strong and a weak version which fits pretty much my own ideas of strong and weak reductionism in general.

[M]aterialism is a form of reductionism, in that it all could be explained through matter. I guess there is a strong and there's a weak form, and with the strong way, I guess you are saying that it really is just matter, and... scientists are, I guess, explicitly or implicitly, got that materialistic approach, haven't they, they have purely naturalistic equations, how matter interacts with matter, through the air, through fields, through spacetime, whatever that is, and all the explanations must be within that kind of framework. (21 Mid Career, Physics, Male, UK)

This broad range of definitions of what reductionism actually is shows that the issue of who supports reductionism and who does not is more complicated than the popular science books suggest. Of course this is predictable in a way because the books only mention reductionism if the author has a particularly strong opinion about it. However, the interviews have shown that many scientists do not in fact have a particularly strong opinion of it. While quite a lot of them have heard of the

concept, comparatively few were prepared to tell me what they thought it meant, and of those a lot qualified their interpretations by either saying that the issue is not really that important and therefore they have not thought about it much, or by being self-conscious about their lack of philosophical knowledge. Conversely, a couple of scientists gave me a fairly confident interpretation of reductionism, which however did not tie into anything like the philosophical concept (not even in the “methodological” sense of Rose), and probably was more due to a misunderstanding of the term than a different interpretation of it.

A couple of points here are worth noting: the people who have contributed their own definitions of reductionism are predominantly from the biological science rather than from the physical sciences, which may well reflect the idea that biologists have more to lose from reductionism than physicists, because their discipline is under threat of being reduced away (in the manner of natural history being reduced to molecular biology in the development that Ernst Mayr feared so much, see section 3.2 above). By this I do not necessarily mean that the biologists I interviewed really were afraid of reductionism, because many of those quoted reacted quite favourably towards it: However it may mean that through the debate that has happened in at least some corners of the biological sciences the term reductionism may have slipped into the everyday scientific language in a way that it maybe has not for physics or chemistry.

Another feature to note is that in this section there is not a single reference to a French scientist. The reason for this is not that the term was less recognised in France, in fact I found quite a lot of respondents who nodded and said that yes, they knew about it in France. Here there is possibly a note to be made about how I asked the question, because the interviews in France were on the whole conducted towards the end of my study, and I had generally abandoned the question that asked my respondents to define the term by then. This was because it often drew a negative response with people being unwilling to commit themselves even if they told me that they had heard of the term. However while a couple of the above quotes did indeed come from a direct question of mine about how they would define reductionism, most were offered spontaneously, and that for some reason did not happen in France.

#### *4.2. My exposition of reductionism and reactions towards it*

In the general absence of agreement over what reductionism is, and in what terms it should be discussed, I attempted to give an exposition of what I personally think are the most interesting aspects surrounding reductionism, and then asked the respondents what they thought about it. I will start this subsection by stating the way in which I asked the question about reductionism, based upon my impressions from the popular science books, and then chart the responses towards it.

Usually my introduction to reductionism would start by describing how some people would argue that a successful explanation in a “higher” level science such as biology or even sociology would make reference only to terms from lower level sciences, so that biology effectively gets explained by chemistry, chemistry gets explained by physics, physics gets explained by elementary particle physics, and so on. I would then draw in two aspects of this idea, first I would point out that this has been seen as a question of explanation: what features must a good explanation have, and why? Here the question surrounding reductionism is whether we really always need to go a level down or two to have a successful explanation. Connected with this is the question of what we really mean by these levels anyway. Another question in this context is where do we stop? Quite a few scientists here pointed out that we can fall into the children’s game of asking why after every explanation, though it is not always argued that it is necessarily a bad thing.

Next to that there is the question about whether there are fundamental laws of biology. I have tried to present this question generally as being about laws such as the law of evolution, which has often been used as an argument against reductionism, because evolution works on every imaginable population that is governed by things that behave like DNA: real life, computer simulations, or even hypothetical silicon-based aliens. In all these cases the underlying material molecular basis of the population is completely different, and it has been argued that therefore the evolution of a population cannot be reduced to the behaviour of the molecules that make up that population. A couple of physicists asked me to give them an example from physics, in which case I talked about the lack of success that physicists have so far had in explaining thermodynamics and its arrow of time laws through the time-symmetric laws of statistical mechanics.

Before describing the interviewees' reactions towards my version of reductionism and the questions it raises, I wish to briefly point towards how it relates to the categories of reductionism that I encountered and argued about above, in the popular science books. In section 2 above I distinguished between different types of reductionism by a) what they say is being reduced, b) whether reductionism is meant to be about the way the world is structured (ontological reductionism) or whether it is about how science should operate (methodological reductionism) and c) whether reductionism forces us to always be reductionist or merely strive to be reductionist wherever possible.

The way I introduced reductionism in the interviews was meant to draw out what I think are the most interesting questions the concept raises, those about the nature of explanation and whether there are fundamental laws in the higher sciences. In the scheme outlined above, the reductionism I introduced was about facts being explained by facts from lower level sciences, though it does not quite rule out the interpretation of theories or hypotheses being explained. The question whether there are fundamental laws in biology or chemistry or whether all fundamental laws should be physical is again open to interpretation in both the ontological or the methodological way, depending on our attitudes towards the nature of laws or fundamentality. The interviewees therefore were free to give their own interpretation of reductionism within the schema described here, and at the same time I asked them what I think are substantive philosophical questions nonetheless. Regarding the third distinction, which I have argued above is the most important in my analysis of the differences of opinion (and identity) in the popular science books, this is included implicitly in the question when I described reductionism and asked whether they think science works like that. Some people replied it always does, others that it only does under certain circumstances.

### *Initial reactions*

In discussing the initial reactions from some scientists here, I cannot avoid some degree of overlap with the following subsection, because some of the categories I will later concentrate on have come up immediately in the reactions to reductionism with some of the respondents. Here I concentrate on who reacted positively and who reacted negatively towards reductionism, and present some of the arguments



they presented in support of the reaction, in the later section I will point back to some of these quotes here (and even in the previous section) to support some of the philosophical categories I will discuss in more detail.

Some people reacted positively towards reductionism but argued at the same time that there is more to science. Here their argument is similar to the “weak” reductionism encountered in the popular science books:

I believe that, you know, large areas of science are reductionist. Now, when we come to biological systems, then I am actually a believer in trying to find as much reductionism in the system as possible. To the extent that I believe that in some measure living systems can be explained by the chemistry of the components. (6 Senior, Chemistry, Male, UK)

But he also went on to explain that he is a great believer in emergent phenomena which he said can “never be explained in terms of the lower level”. The existence of emergent phenomena is usually one of the main arguments of people who argue against reductionism.

One theoretical physicist took the more radical position that everything can be reduced to fundamental laws, but that in practice this can take up two different forms depending on where you start: While the biologist may start with something that needs explaining and works his/her way down, the physicist starts from the other side and works up. However, after this interesting but decisive endorsement of reductionism, he went on to qualify his support by remarking that in practice, there is no rigorous way of doing such a reduction.

It's not that in theory these things can't be done, it's just that there is no rigorous way to get from the underlying fundamental laws up to the level at which you can deal with practical problems. (23 Early Career, Physics, Male, UK)

The almost ontological expectation of reductionism that everything is ultimately explainable with elementary physics was expressed quite often, even by people who were not so enthusiastic about reductionism's value for science. One

typical reply to my explanation of reductionism and the follow up question whether everything is ultimately explainable by elementary physics was: “It should be at least” (34 Mid Career, Physics/Chem, Male, France). Finally for some the question did not make much sense because everything is ultimately made out of atoms, and I struggled to convey the philosophical problem behind it.

Yes, I... [*indaudible*] are very complex organisms, and we can't understand everything now, but I think everything is chemical or physical inside, and we just react, that's... but again that's because I am too much scientist, I don't know. (26 Early Career, Biology, Female, UK/Fr)

Here there is also a hint of the “truth will out device” (TWOD, discussed by Gilbert and Mulkay 1984 and above in chapter 2), because she is arguing that while we may not know *now* exactly how everything reduces to chemistry and physics, we expect that we ultimately will. However in this case I suspect a more philosophical TWOD here – she is not necessarily expecting that we will find out, but she is expressing a philosophical expectation that the world is like that, whether we find out or not.

There were fewer reactions that were mostly negative to the concept of reductionism, though overwhelmingly most people were more cautious without damning the idea outright. Of the negative reactions, one biologist was responding to his own definition of reductionism (respondent 11, quoted above), which was about reducing experiments by controlling the variables in the experiment, similar to the definition of reductionism given by Rose (quoted above in section 3.2). His objection was that it oversimplifies how biological organisms really work by manipulating them outside of their natural environment, and therefore producing knowledge that is, for practical applications at least, useless.

That the one version of reductionism that even Rose found some appreciation for is rejected by this biologist is interesting. Even more so considering that this scientist was not actually unsympathetic towards the version of reductionism as I have explained it later on in the interview: “I can see that, yea, I mean I've never

thought about science in that way, but I can... that makes sense to me.” (11 Mid Career, Biology, Male, UK).

But then there were also opinions that there really are things that are not reducible to atoms, or not explainable purely by the behaviour of atoms.

Yea, probably, I don't think you can break things down quite so far if you're looking at entities... complex entities, because they do behave in different ways. And ok, you may say that the individual particles and so on that go into making up that person are... the basic building blocks and that may be true, but you can have a dead body and you still can't make it work, just by having the same collection of particles that five minutes earlier had been alive. So, actually, no I don't think it goes that far. (8 Senior, Biology, Female, UK)

It does not actually have to be a mysterious concept such as consciousness, life or astrology that shows that you can not explain or describe everything with molecules or atoms. Some scientists have pointed out that even everyday concepts may make no sense on a molecular level.

It's like taking a big selection of cakes, and all the cakes are different, but if you say they've all got atoms in, so what? That doesn't tell me whether it's a cherry cake or a walnut cake, you know? (24 Mid/Early Career, Biology, Female, UK)

A slightly different but much more prevalent reaction was not to argue that there are things that are not explainable through the behaviour of individual atoms, but that the complexities involved will become so great so quickly that reductionism becomes impractical, and therefore useless for science (for example 19 Mid Career, Chemistry, Male, UK).

#### *Are there fundamental laws in biology?*

There were several views on whether there are fundamental laws in biology (or other “higher” sciences, by which I also mean thermodynamics, as opposed to

statistical or even quantum mechanics). Some respondents thought that there are different levels of explanation, and by extension that there are indeed fundamental laws for the higher sciences. This can take the form of a practical position, meaning that things become very complex very fast, and that these higher fundamental laws take on the form of statistical laws or approximations (for example the position taken up by respondent 19 shown above).

For example is it possible that you could take Schrödinger's equation and come up with the theory of evolution? And... I don't think you could, I think there are too many branches of, mounting back upwards, to actually come up with evolution, I think that must be an incredibly difficult thing to do, in the reverse way. (32 Senior, Physics, Male, France/En)

So even though it was almost always held by the interviewees that in principle it is possible to reduce everything to molecules, atoms and even wave equations, that does not mean that our understanding of what happens on a higher level is dependent on that reduction. As respondent 23 quoted above argued, a reduction can be done by starting from physics and working up to biology, but also the other way around.

But it can also mean for some people that there is something more intrinsically fundamental about higher laws, and about science itself.

If you know about quantum mechanics and the fundamental principle in Newtonian dynamics, that doesn't mean that you will get to understand the perfect gas law. (33 Senior, Physics Male, France)<sup>35</sup>

Here this is about understanding, or comprehension. The point is made later on in the interview, that we do not need to know about fundamental particle physics to understand laws that are higher up (this scientist explains that it even applies to himself: his specialization is materials science, and the last time he seriously thought about quantum mechanics himself was during his undergraduate years).

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<sup>35</sup> si vous connaissez la mécanique quantique et le principe fondamental de dynamique de Newton, c'est pas pour autant que vous arrivez à comprendre la loi du gaz parfait.

I already quoted respondent 8 above, who used a very different kind of argument to the effect that there are fundamentally some things that cannot be explained by physics. A more extreme version was (in my opinion quite surprisingly) made by one scientist, who practises astrology in his spare time. Here opposition to reductionism is quite consciously made to allow this scientist to argue that although there is no way (that we know of) to show that astrology works from physical principles, that doesn't mean there are no other types of law that could explain astrology.

I'm not saying that physics could not be used to a certain degree, but I don't see why we should constrain ourselves to using physics, and, as I have said [earlier] anyway, I practise astrology, and I have noticed that you can get something out of that.(37 Senior, Biology, Male, France)<sup>36</sup>

That kind of argument was not very common, though some people have more or less half-heartedly conceded that there is at the moment no way to explain some phenomena with physics, such as consciousness, though mostly people did not think that meant there would be a completely new level of explanation required to understand such a phenomenon.

The reverse is also arguably possible, i.e. that we may need the higher sciences to understand physics. This argument was not made on its own, however one scientist argued that the explanation and understanding work both ways:

My own feeling would be that in fact evolutionary theory for instance a global event, is absolutely necessary, but it can't totally neglect what happens at the molecular level, so... you have to look at the kind of articulation between both descriptions, but you can't only imagine for instance you have variation in genes and you know nothing about what these gene are, so you must somehow enter into the molecular details, but if you

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<sup>36</sup> Je dis pas que la physique puisse pas être utilisée jusqu'à un certain degré, mais je vois pas pourquoi on devrait se contraindre à utiliser la physique, et, comment je l'ai dit de toute façon, je pratique l'astrologie, et je fais le constat que on peut en tirer quelque chose

have these molecular details, nevertheless you need the higher theory to understand what happens, so but you can have... totally separate vision, you must try to... try to relate them, both descriptions. (36 Senior, Biology, Male, France)

On the other side, there were plenty of scientists who have argued that indeed, there are only fundamental laws of physics and everything else derives from them. I have already quoted some opinions to that effect in the initial reactions subsection above, for example the scientist who used a TWOD-like argument to show that she expects everything to be ultimately explainable with particle physics. Another very typical reaction was the argument that all organisms have to obey the same laws of physics that atoms have to obey.

your biological cell has to obey  $F=ma$ . If you could show me a biological cell not obeying  $F=ma$ , then I'm interested. If you can show me an atom in a biological cell or a molecule, not obeying  $F=ma$ , well then, please show me. (21 Mid Career, Physics, Male, UK)

To many scientists who have interpreted my question in that way, the whole idea of being against reductionism seemed dubious, because they thought it somehow meant that biological organisms do not obey the laws of physics. Often then, I had to introduce the argument about evolution that I introduced at the beginning of this section, i.e. that the laws of evolution work on all manner of different types of molecules, even on virtual molecules in computer simulations, and that therefore (some people have argued) there must be something fundamental about evolution that is not explainable through the movements of individual particles. But even so, some scientists were dissatisfied with the idea, and thought that being against reductionism means being against the fundamental physical laws:

But I don't see how it disproves... it doesn't disprove the fundamental laws, it just, again is an example of it being more complicated than we could hope to model at the moment. (15 Mid Career, Chemistry, Female, UK)

Within the group of people who feel that everything is explainable by physics, there is also sometimes a hint of a sort of *anti*-TWOD: the resigned feeling that, although the scientist has a philosophical conviction that everything is reducible, we may never actually find out. That in a way is not unrelated to TWOD-like arguments, as argued by respondent 26 above, but as a philosophical conviction rather than an optimistic view of the future.

That becomes a more theological question... I would say, for me, if you ask me personally, I'd say yes. No well... will that happen one day or will it be proved? Probably not. So then that's not really scientific. (34 Mid Career, Physics/Chem, Male, France)

Finally, I would like to finish the discussion of fundamentality by pointing to one very perceptive comment which in my opinion leads to a very complicated problem located right in the heart of the philosophical debates about reductionism, and which has led me to question whether there is not a hint of circularity in the very foundation of the concept. Even though this scientist was quite sympathetic to the idea of reductionism, he argued that there is no reason why physics should have a monopoly on fundamentality.

[A]ctually who does, who do the fundamental laws belong to, I mean as physicists we don't have an exclusive right to derive fundamental laws. (23 Early Career, Physics, Male, UK)

This insight leads to a complication regarding the nature of reductionism, however, because if we define physics by its fundamentality, then reductionism is true, but only trivially so, because even if we find out that evolution is a fundamental law, then it would simply become part of physics. So the question becomes one of whether we would be happy to see evolution as a law of physics, and this is more about individual preferences about what role in science and in the individual disciplines fundamental laws play instead of a discussion of the nature of either science or the universe. It may then end up that reductionism becomes a question of disciplinary boundaries rather than a philosophical question about

science or the world as such. Maybe this would in fact explain quite well how reductionism has become such a disputed concept within the Nature/Nurture debate as I have shown earlier in this chapter.

*The role of reductionism in science: does it actually matter?*

I quoted some scientists above who argued that reductionism is fine, and that the world really is structured reductively, but that we may never find out exactly how, or that this is simply a philosophical conviction that remains unprovable. Reductionism is fine in theory, but practically unimportant.

If mathematicians and theoretical physicists can't even solve the three body problem exactly, this is not much good to those working on complicated organic molecules. Likewise, what I do in my laboratory will have some bearing on what an evolutionary biologist does, but in practice it's not helpful to them on a day to day basis. (15 Mid Career, Chemistry, Female, UK)

The feeling that reductionism is merely a philosophical question (in the colloquial sense of the term), pervaded the interviews. Among people who had not heard of reductionism or the underlying concept this may not be particularly surprising, but even most of the people who had heard of reductionism and have developed strong opinions for or against it have told me that the problem is actually not really important.

I'm not too excited by, by these grand claims about the general nature of science and, and then you're going from theories of everything in particle physics to general statements like that, so I wouldn't, you know, it's not a debate I would get involved in, either one, to defend one view or another, because I don't, I'm not sure it's really fruitful. [...] Fruitful in the sense that you know, does it bring something new to, to scientists that they can use in their everyday work? I don't think so. (16 Mid Career, Physics, Male, UK/Fr)



Interviewees have often told me that the issue does not really matter, or that they do not have a strong opinion on it, or that they have never really thought about it. All of these responses suggest that both reductionism and the concepts it stands for are not really considered important, even if people agree with it.

My explanation of why I picked out this topic for the interview has revealed at least some kind of agreement that the subject could be of importance for *other* people at least. Often in the discussion I tried to explain some of the passion that reductionism has inspired, taking the popular science books as a frequent example. The suspicion that there may be an element of disciplinary rivalry here was even echoed by one chemist when he was talking about the discipline of systems biology and their attitude towards reductionism. Responding to my explanation of reductionism in the Nature/Nurture debate, he immediately thought of the argument about reductionism in systems biology.

Systems biology. Yea, it's a big buzzword, research funding thing, across Europe, and I'm sure everywhere else around the world... and what... but I think it probably, I think the argument is raging about what systems biology means, I think some people interpret it like 'you chemists get your hands off our biology' (17 Mid Career, Chemistry, Male, UK)

Systems biology was in fact mentioned by two other scientists both in the context of reductionism, so some scientists at least argued that reductionism may be relevant for some people, even though almost nobody actually argued that it is relevant for them.

One exception would possibly be respondent 12, who I quoted earlier as having had arguments about reductionism with a (staunch anti-reductionist) colleague of his. But even he argued that reductionism was used more as an insult rather than a philosophically coherent philosophical viewpoint, and that therefore the practical usefulness at least of the discussion was limited.

## **5. Discussion and summary**

I found the reception of reductionism in the interviews very interesting given the forcefulness with which the issue of reductionism gets debated in the literature

about sociobiology and evolutionary psychology and some of the physics books. Some commentators, for example Wilson in *Consilience* even went so far as to suggest that reductionism is the essence of scientific method, and that without it the practice of science would be impossible, while others, argued that reductionism is a philosophically dangerous practice that simplifies the problems under study by concentrating too much on the detail in neglect of the bigger picture.

I will concentrate the analysis in this chapter first on the evolutionary psychology/sociobiology books discussed in section 3, focussing in particular on the contrast between pro- and anti-sociobiology books, and the development of reductionism in the works of Wilson. Then I will widen the discussion to comparing the books with the interviews, and discuss what I think are some of the conclusions for the study of popular science.

### *5.1. Reductionism as a philosophical identity*

The differences that I pointed to in the books on the philosophical *opinions* about whether reductionism is a good thing or not do follow the divide between pro- and anti sociobiology/evolutionary psychology very accurately. Even Wilson, who wrote *Sociobiology* (probably quite naively) within the climate of the debate on Nature vs. Nurture already in the background, seemingly only became a reductionist once he entered that debate. And yet, through the variability in what reductionism can be interpreted as, and who actually counts as a reductionist, the actual opinions about scientific method do not seem to divide along the lines of who thinks of himself as a reductionist, and who does not: Mayr and Pinker for example relate compatible viewpoints, while the anti-reductionist Rose identifies a form of reductionism he agrees with. In this section I will explore the situation as an arena for a social identity issue, drawing largely on social identity theory as described in chapter 3.

### *Philosophical opinions and philosophical identities*

To fully understand where exactly each of the quoted popular science authors stands in the philosophical framework outlined above, would require a much more thorough analysis of their work. Even then, words like theory, explanation or complexity, all contentious philosophical concepts, are not unproblematic

themselves. A further point to note is that, with one exception (Wilson 1971) the works I quoted were more or less popularizations of science. If the reader has to sift through philosophical literature to understand a concept that the authors attach obvious importance to, then the popularization has failed. For that reason I will take the quotes at face value, even if this is not always completely fair to the authors, as I suspect that some formulations of reductionism suffer merely from poor or thoughtless presentation.

There really do seem to be some fundamental differences about both how science should work and about how the world is structured, although these differences do not come out very clearly in the author's discussion about reductionism, particularly because these divisions do not run along the lines of who calls himself a reductionist and who does not. If we take reductionism to mean strong reductionism (i.e. that *all* explanations have to be of a reductive nature), as most of the anti-reductionists argue, then only Ridley is possibly a reductionist (though as I have argued, even then this interpretation of him is very uncharitable), while Wilson oscillates between the two interpretations. On the other extreme, Pinker has ideas about science that are seemingly less reductionist than those of the anti-reductionists I quoted. It is interesting then to notice that at least by the time Wilson writes *Consilience*, he identifies so strongly with being a reductionist, even if his actual definition of reductionism is seemingly at odds with some of the other reductionist authors writing on his side of the dispute. Another curiosity is the similarity between some declared reductionist and anti-reductionist positions, like those shown above between Mayr and Pinker.

The way these authors see and describe themselves as being either pro or anti-reductionism despite the way they actually define reductionism is part of how they see themselves philosophically and how they would like others to see them. The authors are writing popular science which is aimed not just at each other, both pro and contra sociobiology, but also at the general public. This possibly gives extra incentive for the author's positioning of himself as part of a philosophical group which he finds socially desirable to belong to. To construct their social identity and belong to the group, they need to know which values and beliefs they must acquire and convince others that they have them (Bar-Tal, 1998). A commitment or opposition to reductionism could be such a belief, following the rhetoric for the

opposing camps in the sociobiology dispute. Then, rather than being a mere philosophical or methodological commitment, reductionism or opposition to it marks the authors' membership of what they perceive is a socially desirable group.

Social identity theory has also in part been developed to make sense of social comparisons. The individual self-esteem of members within a group is satisfied by "maximizing the difference" (Hogg and Abram, 1988 p.23) with outsiders. Likewise, differences within the group can be downplayed. Seeing how strongly the opposition to sociobiology identified Wilson and those defending him with reductionism, it is very possible that he started identifying with being exactly that. Thus it was possible for Wilson the holist to become Wilson the reductionist, without him needing to change his opinion.

This development adequately describes how Wilson could have come to see himself as a reductionist. It still leaves open why Wilson did not revise his actual opinion on the matter, which, as demonstrated, is not so very much different to other people who do not think they are reductionists. In other words, although Wilson's idea about what reductionism is may well have changed through the mechanisms of social representation and social identity, his underlying philosophy seems to have stayed fairly constant.

### *Conflicts between philosophical identity and philosophical opinion*

As shown above, many people, especially the critics of reductionism, associate reductionism with determinism, materialism and atomism. This fact is not only noticed by Levins and Lewontin, as mentioned above, but also Richard Dawkins, who observed, when called a genetic determinist, that determinism "is one of those words like sin and reductionism: if you use it at all you are against it." (Dawkins, 2003 p.197). In an endnote to the newer editions of *The selfish gene* (Dawkins, 2006 [1976] p.331), he also displays his impatience with arguments by his opponents against reductionism, and that the only good arguments they offer are against determinism rather than reductionism. However, he sees himself as a reductionist, but not a determinist.

Philosophical viewpoints very often come together as packages, even if they assert things independently. A commitment to classical logical positivism for example would also entail a commitment towards reductionism (in Quine's sense),

anti-realism and other things. Segerstråle (pp.284-291) examines how the critics of sociobiology typically charged the “Nature” side with being reductionist and how this usually came from an (often Marxist) ideological worldview in which reductionism is a sign (among others) of bad science. Thus, unless they indulge in a specific philosophical criticism, followers of a particular philosophical school might have to accept philosophical views they would otherwise not agree with. This is not merely a case of swallowing a bitter pill. Rather it represents a distortion of what the scientist thinks a philosophical viewpoint, which he/she might not otherwise agree with, is actually about. For example, consider people who see themselves unshakably as a Marxist, and then learn that all Marxists are materialists (suppose they do not really know what materialism is, but it is not something they would agree with if they did), then there will be a tension between their identity as a Marxist and their actual opinion. One possible way of solving this tension is to believe in a watered-down version of materialism that they can support, and believe that all other accounts are straw-men.<sup>37</sup>

In a similar vein, I think there is a tension evident in the popular science writing on the Nature/Nurture debate. As it is desirable for the authors who chose a side in the debate to define themselves philosophically consistent with the rest of that social group as either reductionist or anti-reductionist, they must either change their opinions to fit with what they believe to be reductionism, or adopt a type of reductionism that accords with their opinion. In the case of such an already highly abstract, intangible and contested concept as reductionism, the latter option is certainly understandable.

In the books in my sample this manifests itself in two forms. First, of course, the author can be unaware that there is any difference of opinion regarding the definition of reductionism. Second, he can give two or more accounts of reductionism and then choose the one he agrees or disagrees with. This was done by Pinker, who both rejected and accepted different versions of reductionism, and ultimately identifies as a reductionist. Rose also gives an account of a type of reductionism he agrees with, yet identifies himself as an anti-reductionist. Finally, Wilson’s gradual transformation towards reductionism makes sense. Instead of

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<sup>37</sup> See also cognitive dissonance theory (Harmon-Jones and Mills 1999) for an account of people’s strategies on dealing with conflicting beliefs.

changing his opinion about how science operates, he changed his opinion of what reductionism is, or rather, he probably thinks that he had been wrong about reductionism before, but now that everybody calls him a reductionist, he claims that he was one all along.

## *5.2. The value of reductionism for the disciplines and its implications for the development of philosophical debate within science*

How does the discourse on reductionism in the interviews compare with what was discussed in the books? Much more so than with the other topics discussed in this thesis I found that the way reductionism was discussed in the interviews was divorced from the popular science discussion: Because the discussion of reductionism in the interviews did not suggest its use so much as an identity marker in the same strong way as in the books, reductionism was much more seen within its philosophical merits, or lack thereof. Here the difficulty arose that reductionism has been such a contentious philosophical term both within philosophy and some selected scientific disciplines, that any discussion surrounding the term had to stay as vague as the term itself. Delving into the substantive issues that I think lie behind reductionism in the interviews however inevitably meant that the way we discussed reductionism was always close to my own interpretation of it. However, it seems that, in a way similar to Occam's razor, the way the philosophical issue behind reductionism is discussed by scientists is different to the way the term reductionism is discussed by popular science, and even by philosophers themselves.

This difference between the two ways of discussing reductionism in the books and in the interviews carries lessons for philosophy as well as for scientists themselves. As argued above, in the books reductionism seems to have, next to its philosophical value also a use as an identity marker. By this I do not mean that the authors cynically tailor their beliefs to be able to say that they or their theories support/knock-down reductionism, rather I argue that reductionism, which is already a very sketchy term gets interpreted very widely in a way that makes sense to the scientists who believe that their beliefs are typical for their discipline. Having developed this line of argument I do not wish to belittle the philosophical problems associated with the discussions of reductionism, which are plenty and worth having. The issues, especially those debated in the social sciences which dislike

reductionism “reducing” their explanations to “merely” biological ones, are not just about disciplinary identity and the social positioning of these sciences, as my argument may at times suggest. Instead there are interesting philosophical discussions to be had about the role of fundamentality, what kind of explanation is required to understand a fact; can we always ask “why” to a scientific, never mind biological or sociological explanation, or will we have to stop at some point? What kind of point would that explanation be? These are all interesting, fascinating and important questions for the foundation of the “higher” sciences, and sociologists and social psychologists have in my view correctly identified reductionism as a central philosophical problem within their disciplines. What is more, I would argue that the issue has been inexplicably ignored by too many other scientific disciplines, because the nature of fundamentality, understanding and explanation are important on every level of a hierarchy, which anyway only really makes sense once we have a tentative answer to these questions.

What I argue instead is that the philosophical debate within science has been impoverished by a needless juxtapositioning of reductionism with the accepted party line, coupled with the fact that reductionism itself is such an intangible concept that this has led us into a situation where reductionism means whatever people want it to mean. In this sense it is possibly similar to what has happened to words like democracy, which also has lost a lot of its original meaning through overuse and its co-option as a rhetorical identity marker, all of which of course does not mean that the underlying philosophical issues have lost their importance. On the contrary they are maybe even more important because a substantial part of the vocabulary used to discuss them has become useless, and debate unnecessarily confused.

My survey of asking scientists directly about the issue of reductionism, and the underlying issues that I believe are the most important to flag up, in a way showed that the discussion can be, and is being, held on a completely different level by scientists in their everyday work. The results are of scientists giving me many varying and thoughtful opinions on these topics which, like all the other topics I looked at for this thesis are not as easily pigeonholed as either the popular and philosophical debate often suggests. Next to that there is an overwhelming sense that the issue is really only (colloquially) philosophical. While the questions raised

by reductionism are indeed often recognized as interesting and thought-provoking, they are also often seen as of no practical relevance.

### *5.3. Implications for popular science*

Many of the points raised in the last chapter on Occam's razor about how representative popular science books are of day-to-day science also apply to the case of reductionism. However, there are also obvious differences. Firstly I argue that reductionism has taken on a different rhetorical role in popular science than Occam's razor (as opposed to simplicity) has. Whereas Occam's razor appears as the authoritative demarcation rule between science and non-sense, reductionism has acquired an importance as a recognized debate *within* science, and as such carried into some of the disciplinary disputes that arise from reductionism-related issues but that now trivialise the actual issue. While Occam's razor as well as simplicity-related issues have received attention from all disciplines within the popular science books, reductionism concentrated on the Nature/Nurture books and a couple of physics books. Here the issue of reductionism in popular science becomes interesting because the public is now invited, consciously, into a philosophical debate that even the authors acknowledge, however grudgingly, is not universal opinion among scientists. The closest to that argument is possibly Wilson who argues that reductionism is almost the same as scientific method. But even Wilson acknowledges that while he thinks that is what all scientists should be doing, not all of them are doing it, and therefore reductionism takes on a different flavour than Occam's razor does (or Popper or the positivists do for other authors, as shown in chapter 5), whose proponents assume all science works like that.

This brings us to an unfortunate and probably unintended implication this wild use of reductionism for disciplinary positioning has for the authors in question. Unlike the case of most of the other philosophical topics, readers are caught in a philosophical crossfire with regards to reductionism. While it is unlikely that readers would notice the subtle variations in the meaning and value of simplicity in science, people have definitely noticed the issue of reductionism in the Nature/Nurture debate, with reductionism receiving brief discussions whenever the subject comes up for science studies debate. That poses the danger that people who read such a popularization may already be predisposed towards or against the



position as outlined in the book, on the ground that the author will get stereotyped into a social category (see chapter 3), which conforms to all the prejudices people already hold about reductionists/antireductionists. To give an example, suppose for whatever reason I accept reductionism (I may be an evolutionary psychologist myself), and read a book (unrelated to evolutionary psychology) which argues against reductionism. I will therefore have a reason to dislike the book's argument, regardless of whether I agree with the substantive issue.

To prevent this from happening, many of the authors who do write about reductionism, the physicists included, have written about two or more versions of reductionism, one of them reasonable and the other not. But I think that this is also more than a strategy purely to convince others, it is about convincing themselves that reductionism towards which the correct attitude must be displayed, is actually what the scientist believes. Therefore this rhetorical move is in the end self-defeating, as the opposite rhetoric goes the same way, only inversely, and we arrive at situations where Mayr and Pinker hold surprisingly similar views regarding science, but disagree about what reductionism means. Ultimately this cannot be a good strategy for anyone wishing to convince undecided readers or even scientists from the other side of the dispute to their view. Maybe this is even one of the reasons why Matt Ridley's book *Nature via Nurture* failed to be convincing as the peace offering it was intended to be, because Ridley kept using, and defending the offending vocabulary, even if (as far as I can judge) the actual content of the book was not meant to be confrontational.

How this heartfelt debate about reductionism and its role in science looks like to scientists outside the disputes in which reductionism has taken on this role, is aptly illustrated by the scientists I talked to. Most have expressed interest in the underlying issues, even if they regarded them as fairly philosophical in nature and removed from day-to-day importance. The word itself insofar as it was used at all, was not often recognized as being overly contentious, and I was told by one scientist who *did* know about the importance that reductionism has in the Nature/Nurture debate but was otherwise uninvolved, that he thinks reductionism is more or less a storm in a teacup.

### 5.3. Summary

Reductionism in the popular science books was very prominent in books by authors that were involved somehow in the Nature/Nurture debates, and reductionism was described in these books in slightly different ways. By comparing them to books outside of my sample from the other side of the debate, and looking into the personal development of one particular author central to the controversy, I developed the argument that reductionism has become a way of identifying with a particular side of that debate (while there are hints that similar things may also be going on in the way reductionism is discussed in the physics books and in several debates in the social sciences as well). I have argued that the concept of reductionism has, probably through this and similar debates, become very nebulous in its precise meaning, so that essentially scientists manage to interpret reductionism in a way that allows them to hold philosophical opinions that can even be seemingly at odds with other people from the same side, or very similar to people from the opposing side.

With the case of reductionism I have tried to argue about the scientists' *boundary work* more exclusively through the ideas of *social identity* than in the previous three chapters where I have shown that philosophies and in particular Popper, Kuhn and Occam's razor are used for boundary work as well as being identifications. I believe that here the interpretations stemming from social identity are more naturally useful in understanding the dynamics with which the belief in reductionism (or anti-reductionism) develops, for example in the works of Wilson, or in understanding the different interpretations of the concept in the different camps that nonetheless can be very similar as to the way science should work.

The interviews suggest, similarly to the other topics in the thesis, that scientists' opinions on the underlying philosophical problems are varied and nuanced, and that there is a substantial amount of reflection about these topics going on within science. Reductionism itself however, while the philosophy behind it are seen as interesting and fascinating, is rarely seen as an important or defining problem, which supports my argument that reductionism has become an important identification mark for the disciplinary groups of authors that so visibly fight over it. This use of the term I have argued may have as a consequence a considerable impoverishment of the scientific philosophical discourse surrounding the interesting

issues behind reductionism, which impacts negatively on science itself, and on scientists' communication efforts. Similarly to Occam's razor in chapter 6, though maybe less pronounced here, the scientists' understanding of and discourse on reductionism also impacts on philosophy and the philosophical discussion within science.

## Chapter 8: Conclusions

|                                                                                   |     |
|-----------------------------------------------------------------------------------|-----|
| <b>1. Introduction</b> .....                                                      | 263 |
| <b>2. Main themes</b> .....                                                       | 265 |
| 2.1. <i>Philosophical authority and the representations of philosophies</i> ..... | 265 |
| 2.2. <i>Boundary work and philosophical identities</i> .....                      | 271 |
| <b>3. Lessons</b> .....                                                           | 275 |
| 3.1. <i>Lessons for social study of science</i> .....                             | 275 |
| 3.2. <i>Lessons for PUS and science education</i> .....                           | 278 |
| 3.3. <i>Lessons for philosophy</i> .....                                          | 279 |
| <b>4. Concluding comments and future directions</b> .....                         | 280 |

### 1. Introduction

This concluding chapter will summarise some of the main themes and conclusions that I have drawn in the previous chapters, and as such it will also compare the material of this thesis over the two studies and the four topics grouped along the major interpretative themes.

The themes focus on scientists' representations of philosophical topics in different ways: The first section will analyse the philosophical representations as boundary objects which scientists use to communicate across the boundaries of different groups. These can be the boundaries between the scientists and their audience, as is most noticeable of course in the popular science books, but they can also be the boundaries between different disciplines and therefore facilitate the communication between the groups. One way in which a philosophy becomes a boundary object is by presenting the philosopher as an authority figure. It is assumed that the reader will recognise that philosopher as an authority and this will therefore allow the reader to accept the author's point about something being proper science without needing to go too deeply into the philosophical argument. Section 2.1 looks very briefly at some of the ways in which the representations of philosophy can develop, how they are formed through the philosophical education of the scientists and how that is reflected in the difference between France and the UK (or rather the relative lack thereof), and finally how these representations in turn possibly influence philosophy itself. The second section recounts my arguments about the representations of philosophy as identity markers, where scientists take

philosophies to define their own identities as scientists in general, or as members of a particular discipline or scientific grouping. The analysis in terms of boundaries is used next to the one on identity, boundaries this time in Gieryn's sense of boundary work. However here it is important to point out that the boundaries are different ones to the ones mentioned above in the context of boundary objects: Rather than being the boundary between two groups who need to find a common reference point, such as popular scientist and audience, these boundaries are the ones the scientist needs to build up, such as the one between science and pseudoscience, or between sociobiology and its detractors. As I pointed out in chapter 3, this use of the same boundary metaphor by Gieryn and Star and Griesemer is potentially confusing, and this is one of the reasons I prefer the identity approach.

These main points will answer the main research questions of my thesis identified in the first chapter: *How do scientists view the philosophical topics on the nature of science, How do these topics get used in scientists' discourse, and How do these topics shape scientists' thinking about science.* It will also address the more subsidiary questions of *what are the differences in opinion and use of philosophy between the books and the interviews, and what are the differences (in the interviews) between UK and France based scientists.*

The second part of this chapter addresses points where the topics raised in this thesis are of relevance to the philosophy of science, sociology of science and the social study of popular science and the disciplines of PUS and in a more limited sense that of science education. Although this thesis is intended to be of interest in its own right, the relevance to the above disciplines is real, and in light of the previous studies of scientists' and the public's implicit epistemologies made by both PUS and science education, as discussed in chapter 2, this thesis makes an important contribution. section 3 will answer the final research questions for my thesis, *What relevance does the study have for social studies of science, the philosophy of science and the fields of public understanding of science and science education?*

## 2. Main themes

### *2.1. Philosophical authority and the representations of philosophies*

I have argued that scientists' representations of philosophy can be seen as boundary objects in the sense of Star and Griesemer, especially when the philosopher or the famous philosophical principle gets used as an authority with which a book author can demarcate science from non-science without having to elaborate on the actual philosophy in too much detail.

This section is split in two, with the first part concentrating on the boundary object aspect of the philosopher as an authority, while a more disparate second part looks at more general issues to do with the scientists' representations and how they develop. This will lead on to section 2.2 which examines the philosophical representations as boundary work and identity markers.

#### *Philosophers as boundary objects*

In this thesis I have argued that the invocation of several philosophical topics, especially when explicitly recognised as philosophical topics, can act as authoritative devices to close off an argument. The philosopher becomes a boundary object; here I do not mean the boundary between science and non-science, but between the scientist and his/her audiences, both public and fellow scientists. It is often assumed that the reader (in case of the books) or the listener (in case of some of the interviews where Popper or Occam's razor performed similar roles) is familiar with what the philosopher or philosophical principle stands for. This may on the surface be surprising, in a popular science book at least, because this genre is explicitly all about explaining science to non-experts. The famous philosopher, and the famous philosophical principle, is therefore a kind of boundary object in Star and Griesemer's sense (see chapter 3), which acts as a common reference point between the author and the reader – even though the reader is not assumed to be familiar with the particular science that is being discussed, he/she will recognise, or is supposed to recognise, the appeal to Popper or Occam's razor. Similarly most of the other remarks on the nature of science as shown in chapter 4 are far too short to convey real information about how the author really thinks science works, yet the philosophical key words such as replicability, rigorousness or hypothesis-testing fulfil the role of boundary objects themselves, because it is often assumed that the

reader is aware of the important role they play in science (in the opinion of the author).

In this role of boundary object the rather abstract nature of philosophical topics helps, because so very often these topics are very much open to interpretation. As I have shown with the example of Occam's razor in chapter 6, there are (at least) three fundamentally different ways in which its usefulness can be interpreted. One of them, the methodological razor, seems to be a version to which almost (but not quite) every scientist could agree on. On the face of it, then, appeals to Occam's razor would satisfy the needs of a boundary object, because everybody would agree with the statement, even if they do in fact have different opinions on the substantive point at issue. In this respect my conclusions about Occam's razor, or Popper are different to my conclusion about reductionism in chapter 7 only because there are two scientific groups that disagree on whether reductionism is a good thing or not.

As argued in chapter 3, it may well be fruitful to interpret the concept of boundary objects through the notion of social representations in Moscovici's sense. In this case scientists' interpretations of Popper are *anchored* in their understanding of good science. There is an expectation that Popper described science correctly. This may be at least partly explained by the fact that some of the great scientific heroes such as Medawar (1979, 1996), Peter Mitchell (the main subject of Mulkay and Gilbert's 1984 study and well known as a supporter of Popper's philosophy, see Morange 2007) or to a lesser extent Hawking (chapter 5) endorse Popper. People's expectations of what Popper really meant in his philosophy may then shift to represent more how they themselves understand science. This is reflected by Deutsch defending Popper against *his own* criticism by essentially arguing that Popper either did probably agree or in any case would have agreed with him if he knew about these points. Similarly in the interviews I often found support for Popper, coupled with an admission that they have not really read him very attentively or not at all. The high, though by no means universal, acceptance rate of Popper is also interesting to contrast with the high frequency with which the scientists in my study have had ideas about science that would have clashed with Popper's philosophy – acceptance of induction or even simple fact-gathering as an important part of science, confirmations and verifications, as well as some people

who agreed with the notions of paradigms and even people who believe that science is not in the business of discovering truth (chapters 4 and 5). All these ideas were often held in conjunction with a vague or conditional endorsement of Popper, though it is interesting that in the interviews rarely anybody went as far as claiming that they knew what Popper *really* stood for. All this was among the scientists who did not reject Popper. There were also some scientists who argued against Popper, and they were invariably among those that have actually had a more thorough philosophical education.

I would even add Sovacool's (2005) study of astronomers' support for Popper (see chapter 2) into this discussion, as it illustrates how a mere support for "hypothesis testing" has so easily been interpreted as support for Popper, both by the scientists in the study as well as apparently by Sovacool himself. A vague notion of hypothetico-deductivism seems to be the most prevalent thought about science both in the books and in the interviews, which supports the idea that Popper has been co-opted to stand for whatever people think is good science. There are of course characteristics of Popper's philosophy that do not get lost, like hypothetico-deductivism, however they become so watered down as to be a lowest common denominator "anti-straw-man" that many people would find it hard to disbelieve in, and therefore even perpetuating the idea that Popper is just good scientific sense.

Kuhn is in many ways different from Popper as he does not stand for scientific rigorousness as Popper does, and that is probably the reason why discussions of Kuhn usually took a different flavour. Unlike Popper, who stands for scientific orthodoxy, and who in the books often got invoked in those pithy philosophical comments which tended to reason why a particular thing was real and proper science (chapter 4), Kuhn was used to support the unorthodox scientists. In the case of Naughton's chapter on Kuhn it explained why some reasonable idea was not taken up (it went against the paradigm of the times); in the case of Grand it actually supports a larger unorthodox philosophical position which Grand sets out in the first chapter, and which possibly reflects the idea that Grand himself as well as his subject are something of an outsider from the hard sciences, and therefore in need of philosophical justification.

Kuhn therefore fulfils a (slightly) different role as a boundary object, where his philosophy is used to appeal to the less powerful scientists and scientific ideas, a



fact which is illustrated by the way in which he is used not just by scientists but also by outsiders of science such as the creationists (chapter 5), who take Kuhn's philosophy as a legitimization: the common caricature of the crank saying "they laughed at Galileo" illustrates the way Kuhn has often been used. Kuhn as a philosopher and historian of science acts therefore also as a "common coin" between the author and the audience, which may not know enough about the science in question to judge its merits, but understands that it is valid according to the philosophy of the renowned historian. To support this, the authority of Kuhn needs possibly slightly more explanation than Popper (or Occam). The differences in this respect between the uses of Kuhn and Popper are not that great, as they are both used to argue for the legitimacy of a particular piece of science; where I suppose the main difference lies is with the relative perceived status of that science.

#### *Some issues on the dynamics of philosophical representations*

Bauer and Gaskell (1999) have visualised the social representations concept into what they call the "Toblerone model" (see also chapter 3): The relationships of the concepts that influence a social representation can be pictured as a triangle between (at least) two subjects that hold the common representation of an object. To emphasise the dynamics of a social representation, where the representation also develops with the "pragmatic context of the social group within which the representation makes sense" (p. 168), Bauer and Gaskell propose to look at how the triangle develops in time – the mental picture that results is that of the triangle being dragged along the time axis, resulting in the form of the packaging of a Toblerone chocolate bar. And because there are different social representations of the same objects, by different people and groups of people, they even prefer to think of a stack of Toblerones, which itself is only an approximate picture because there would be overlaps, twists and different number of Toblerones at different times. Thus they suggest that the picture of social representations is more complex when considered in the context of changing circumstances. Similarly it is worth pointing out how the representations of philosophical concepts by scientists change, what these changes are influenced by, and how they compare over the different groups.

One way is for example by scientists joining and leaving different social and disciplinary groups. As I have argued above, concepts such as Popper's philosophy

or Occam's razor have taken on a role which is beyond the actual philosophical opinion of scientists. The way people think about such concepts fits their own actual philosophical opinions, so for example someone following a mere methodological razor may think he/she supports Occam's razor as a philosophical principle, when they only hold to a view that many philosophers such as Popper or Occam himself would not recognise as Occam's razor (see my discussion of the methodological razor in chapter 6). I have argued that in these cases, scientific opinion is often generally in agreement. In a case where there is considerable difference of opinion within science, joining or leaving different groups may affect how scientists think about these topics. I have argued in chapter 7 that Wilson's idea of reductionism was influenced by the two different groups with which he identified over time (zoologists and sociobiologists) and their differing attitudes towards reductionism. A physicist (35 Senior, Biology/Physics, Female, France) who moved from straight physics to dealing with neuroscience told me how she thought of scientific method as variable between the disciplines, but then when she joined her current research group she found that the scientific reasoning was fundamentally the same, and that that has influenced how she thought about science generally.

Where scientists' representations of philosophical topics and the nature of science come from is also crucial to how scientists understand them. The education that most scientists told me they received on the nature of science was informal, based on picking up "tacit" knowledge, and mostly conveyed to them by other scientists. Otherwise, the context in which philosophies are picked up varied enormously from self-motivated reading, to attending lectures and even whole lecture courses on philosophy of science, run variously by scientists or philosophers (chapter 4). In each of these contexts philosophy gets communicated differently, and that reflects how the philosophies, as well as the nature of science, gets interpreted.

A point has to be made regarding the comparison between France and UK based scientists. Because I did not find much difference between the philosophical opinions of the French and the UK scientists, one possible conclusion would be that local academic philosophical fashions do not really influence scientific thought. Instead, the scientists' philosophies form part of a international body of opinion in

much the same way that science itself is much more international (and Anglophone) in focus than philosophy is.

However, as I found that the older French scientists in my sample were generally more aware of the French epistemological tradition than younger French scientists, there may be a generational influence here. There are several factors to consider: One is that even in Anglophone discussions of philosophy of science (both in the books and in the interviews), the choice of topics is generally restricted to philosophy of science from usually no later than about the 70's – Popper, Nagel, Kuhn, Lakatos, Feyerabend are all widely discussed, while later developments are largely ignored. As I have argued in chapter 2, this in itself may be a result of the “naturalistic turn” in the philosophy of science, where philosophy of science has largely stopped discussing the grand ideas of demarcation and scientific method which are of interest in this thesis. However, there is also the factor that French philosophy of science itself has lost a lot of its momentum, with contemporary French epistemology having been supplanted either by the adoption of Anglophone philosophy of science, or by the adoption of continental philosophy. The “indigenous” French tradition of analytic philosophy is barely practised any more (as commented upon by respondent 36, see chapter 4; also Bitbol and Gayon 2006 p.4 argue that after 1970 French epistemology took a new turn, to be more varied and open to outside influences).

Therefore conclusions about the influence of local and contemporary academic philosophy of science on the local scientific community are difficult to draw. On the one hand, senior scientists from the generation where French philosophy was still relevant in its own country were more likely to know about it, whereas the younger generations of French scientists did not respond to French epistemology at all. On the other hand, the Anglophone philosophy of Popper or Kuhn clearly has had an impact on all generations of scientists in France.

This possibly reflects the idea that scientists do not generally get their philosophical knowledge from the philosophers in their own university or even academic culture, though in a small way those philosophers may have some limited influence. On the other hand, the scientists' philosophical opinions seem to mirror the general fashion among philosophers themselves, if the naturalistic turn is ignored for the moment: the newer generation of analytic philosophers as well as

the newer generation of scientists in France is learning about Anglophone philosophy rather than French epistemology, and in this sense local fashions in philosophy do seem to have influenced the thought of the local scientific community after all.

Finally one small point I wish to outline as well is that the influence can go the other way, i.e. that the way scientists think about philosophical topics can influence the philosophical debate. One of the most striking aspects of philosophical introductions to Occam's razor was that the philosophers insisted that Occam's razor is something that scientists are instinctively doing, and that this is a valid justification for philosophers to philosophise about it. I have also argued that because at least a lot of scientists' actual use of Occam's razor is not something that a philosopher would find useful or even recognisable, we may have ended in a rather curious situation: Scientists follow Occam's razor (under their own interpretation) because they think philosophers have told them to do this (as it features as part of the scientific method), whereas philosophers philosophise about Occam's razor because it is what they think scientists instinctively use.

Of course there are other justifications for Occam's razor, and of course there are also scientists who reject it in even the least controversial forms, but this representational feedback-loop may at least have a role to play more generally in philosophical representations, and this is one of the lessons I draw from this thesis for philosophers. There is also a slight confusion about Popper's (or anyone else's) normative justifications for their philosophy, because for all the norms they are advocating, they also have to argue that scientists either already follow their philosophies instinctively, or that science has been going about its business wrongly until Popper (or any other normative philosopher) came along. These complications go to the very heart of the debates surrounding the nature and aims of philosophy of science.

## *2.2. Boundary work and philosophical identities*

The authors may have had two possible audiences in mind when writing popular science, the public as well as fellow scientists in the field. There is also a possible third audience to be reached by authors appealing to philosophical authority, and that is the author him/herself. Here I think that a possible reason in dropping the

name of obscure philosophers in a popular science book may be to convince yourself that you conform to what you think is proper scientific practice, even if you are not sure that anybody else will necessarily notice.

Chapter 7 showed that the popular science authors who wrote in the evolutionary psychology/sociobiology and the Nature/Nurture debates had a completely different attitude towards reductionism than other scientists, with the exception of some of the popular physics authors. While the scientists involved in the debates take great care to label themselves reductionist or anti-reductionist, most other authors do not mention it, while the scientists I interviewed overwhelmingly thought that while the issue may be interesting, it is not terribly important. I argued that in the Nature/Nurture debates reductionism or anti-reductionism has become a marker for the identity of the respective communities.

In section 2.1 above I have interpreted the use of Popper and Occam's razor as boundary objects which scientists can use to communicate with each other as well as the presumed intelligent audience that will take the appeal to philosophical authority. I would argue that they also serve as an identification for scientists, because the idea of adhering to Popper may well be a defining characteristic of science in the eyes of many scientists, and the way several scientists both in the interviews as well as popular science authors have talked about Popper confirms this. Thus, for being a scientist, it is necessary for many people that they are Popperians (or a reductionist, or adhering to Occam's razor). This identification issue is helped by the abstractness and even the frequent vagueness of philosophical ideas. They are often very much open to debate and interpretation, and even if that were not the case, it is always unclear how much you have to actually agree with, say, Popper in order to call yourself a Popperian. Because the interpretative elbow room is so great in this respect, people of quite various philosophical opinions are able to identify with (or against) these particular philosophers or philosophies or philosophical concepts. I have for example shown that E. O. Wilson's attitude towards reductionism has changed over the years while he became more and more embroiled in the Nature/Nurture debates, from being sceptical and negative about reductionism to calling himself a reductionist. All the while his actual philosophical opinion has not shifted very much, at least as far as can be seen from his writing. This may be the reason why he now writes that he has always been a reductionist

even in the 1960s. Ceccarelli, in her analysis of Wilson's later work in *Consilience*, argues that Wilson uses a rhetorical tactic in which he describes a strong reductionism which should please the hardliners on the subject, while also describing a much weaker version of reductionism which should appeal to a different audience, and that through this method, Wilson aims to bring together the two audiences. While I would not reject this interpretation, I would at least add that the non-cynical interpretation of this strategy shows Wilson to be of two minds in his ideas of what reductionism means, while his identification with reductionism seems at least very certain.

In a sense a tension arises as Wilson tries to fit around his philosophical ideas of how science should work with his identification as a reductionist. The way that he switches between (what he probably thinks is) a "lowest common denominator" weak reductionism and a stronger form of reductionism with which even he admits to having difficulties is a result of that tension. The rhetorical strategy that Ceccarelli identifies as "polysemous textual construction" Ceccarelli (2001 p.5) may in the end also be a strategy that convinces Wilson *himself* of his philosophical identity as a reductionist.

A similar, though opposite reaction seems to have occurred with Rose, who identifies a "methodological reductionism" with which he actually agrees with, but he still describes himself as an anti-reductionist. Here the identification with anti-reductionism is strong enough for the author to discount versions of reductionism with which he agrees with. In general the pattern was that authors who were anti-reductionist described a very strong and disagreeable version of reductionism, which most people even on the other side would not agree with either. The reductionist authors on the other hand described reductionism in much weaker and more agreeable terms. Not all of these are what I would call anti-straw men either, though Steven Pinker's description of reductionism does come very close. It seems, however, that when the sometimes quite vehement identification towards one side of the debate is considered alongside what the authors have actually written about how they think science really works or at least should work, then there is almost so much overlap that it is impossible to see many connections between their various versions of reductionism.

Similarly, though in these cases there is not such a fascinating pro and anti division amongst scientists, philosophies such as those of Popper and Occam's razor have become identifying characteristics of what (some) scientists see as good science. As such, the actual philosophical opinions about how science works vary greatly even among people who notionally subscribe to Popper or Occam's razor, as I have outlined in the previous subsection of this chapter (see Doise 1998 about the relationship between the concepts of social representation and social identities, as I have outlined in chapter 3).

In chapter 3 I argued that social identity theory, from which I draw my interpretation on what is going on with reductionism in popular science above, is also very close to Gieryn's concept of boundary work. In this case, reductionism is being used to draw a rhetorical boundary around the pro-Nature and the pro-Nurture camps, and to exclude the other camp from what they argue is proper science. In a similar way, the smaller pithy remarks and philosophical asides on the nature of science which I have seen in the popular science books in chapter 4 and some of the other chapters, draw a boundary around what the author needs to assert is proper, bona fide science, or conversely, as was often the case with Occam's razor, what is definitely not science.

There is a benefit in keeping both the identity and the boundary metaphors in the analysis, because even though I have argued that they describe fundamentally the same phenomena, they emphasise different aspects: in the boundary metaphor the emphasis is on differentiating the group's identity from others, while in the identity metaphor the emphasis is on the group's identity itself. Therefore I have followed the identity metaphor when describing the conflict between the reductionists and the anti-reductionists, where my emphasis was on the way the philosophical concept was used as an identity marker, which I do not think has a clear equivalent in the boundary literature. However I found it more useful to talk about boundaries when the scientists draw the boundaries around (what they think at least) almost every other scientist would agree to be science. In the pithy comments, as well as in the more elaborate explanations of Occam's razor, Popper and even Kuhn, the scientists use the philosophy to draw the boundaries simultaneously defining what is science, as well as what is not science. The groups

themselves do not have to have some physical manifestation, and disciplinary groupings such as those of pro- and contra- evolutionary psychology or sociobiology can be thought of as imagined communities, similar to the “invisible colleges” familiar to science and technology studies (Crane 1972).

I also argued in chapter 3 that one of the benefits for referencing to social identity theory in a situation where possibly most work in science and technology studies would have made use of Gieryn’s work, is that social identity theory draws on a lot of experimental as well as theoretical work. While I would not suggest that my dissertation contributes substantially to that body of work, my argument is that by drawing inspiration from such a larger and well worked out body of theory, an STS analysis will be able to draw on it for heuristic value and mark out areas for further investigation where a mere reference to boundary work may not have done so, simply because there is so much previous work and experience to draw on. In this case, I find for example that the idea of reductionism as an identifying characteristic of what the participants considered good science useful, because it led me on to consider the different ways in which reductionism was represented in the popular science books, as well as giving me a reason for understanding why reductionism was not considered such a big deal outside of the debate, even if the interviews have shown that almost all scientists have interesting things to say about the topic.

All this is most evident in the books, probably because in the interviews I invited people to specifically tell me their philosophical opinions rather than having them decide when and for what reasons to discuss philosophical topics. The comparison of books and interviews however recounts interestingly how the philosophies have been put to work in the books, and how they contrast with how scientists represent their philosophical views in conversation.

### **3. Lessons**

#### *3.1. Lessons for social study of science*

From the point of view of general science studies I believe that the sociological study of how scientists think about their activities philosophically is interesting in its own right, as it contributes to a further understanding of how science and



scientists work and think about their work. The philosophical reflections of scientists and how that relates to the philosophical issues debated by philosophers themselves and the discursive work philosophy performs in scientists' boundary work – these areas are generally understudied, as I have shown in chapter 2. Even on those occasions that they have been studied, they have been used to draw almost exclusively philosophical conclusions, either by philosophers, such as Bailer-Jones (2003), or by the sociologists themselves. Both Mulkay and Gilbert's 1981 and Potter's 1984 papers on scientists' philosophical discourse were published in a philosophy journal.

Studying the ways in which scientists talk about the philosophical foundations of their activity is important from a practical view as it allows the social scientists studying science themselves to understand what scientists think are the important issues, how they are thought about and represented and what uses they are put to. This helps in understanding wider, more general, issues in science studies, as it can then inform a possible understanding of what happens when the communication between scientists and sociology of science breaks down. An example is the philosophical misunderstanding between sociologists and scientists during the science wars. Understanding the representations of philosophy by scientists can highlight where the science wars hinge on different interpretations and significances of fundamental philosophical terms.

There are also some specific issues particularly in the social study of popular science which this study highlights, though this may not be a lesson as such, but rather a point to bear in mind when studying popular science. Regarding the differences between the scientists and the popular science authors in the exposition of philosophical topics, while it may in the end not be a big problem for philosophers, there are problems for popular expositions that make liberal use of philosophical asides, or philosophical topics such as Occam's razor or Popper's philosophy as a demarcation tool, or that use topics such as reductionism as an identity marker which essentially talks to other scientists rather than the public. In light of the contested nature of philosophical topics in popular science, it is a pertinent question to ask whether the epistemologies as portrayed in popular science are giving us a consistent portrait of science.

When they are not directly meant to be normative, I do not think that current philosophical discussions suffer too much from the difficulties I pointed out, because it is essentially about discussing different things. In the example of Occam's razor, the philosophical discussion of many aspects of the razor will still be interesting and relevant whatever scientists think: in establishing reasonable rules of inference when constructing an artificial intelligence, or when discussing nominalist metaphysics. However, a thorough analysis of what simplicity really means for science, and consequently how science is really done, will be difficult for the general public at which the books are aimed, especially if the demarcating instances of Occam's razor are coupled with the other often contradicting discussions of the values of simplicity between people like Greene and Kirshner. Or, in the case of Popper's philosophy, if he is credited with holding a logical positivist view, or in the case where some books confidently declare science to be version of common sense, and others that science is nothing like common sense. In the case of reductionism, a different discrepancy is also evident: just as for Occam's razor or Popper's falsificationism, there is a possibly different discourse about reductionism than there is about the philosophical concept of reductionism when it is consciously called that.

On the other hand, unlike the case of Occam's razor, the issues surrounding reductionism were rarely discussed without explicit reference to the term. When the term was used by the popular science authors however, it received a host of different definitions that were fiercely contested. However, scientists in the interviews would rarely comment that the issue of reductionism was seen as important, not the term reductionism and the surrounding controversy, when it was recognized at all, but even the underlying philosophical issue was not deemed to be too important. With all these different ideas and definitions of reductionism that are spread throughout the popular science literature, these popular discourses on reductionism are unlikely to help in popularizing science, because they are neither coherent over the different books, nor are they in agreement with the thinking of most scientists.

On its own these points are of course slightly spurious, as I do not expect anybody to do what I did and pick up 30 popular science books just to read them closely for philosophical inconsistencies. It does however raise some more general

questions about the role that popular science plays with respect to the actual business of science. If the publicly visual face of science can portray science in a way that some if not even most scientists from even the same discipline would not necessarily recognize, is it then still possible to claim popular science as a voice for scientists, or even the “autobiography of science” (Jurdant 1993)?

### *3.2. Lessons for PUS and science education*

How do scientists’ views on the philosophy of science compare with what the disciplines of PUS and science education think it is important to know about “how science is really done” (Durant 1993)? As pointed to in chapter 2, the education of the methods and philosophy of science in this respect has been a concern for science education for a while (as reviewed in chapter 2), a concern which has indeed looked at studies of scientists’ philosophical opinions, but which has not generally been taken up yet in the study of popular science and its role for science and the public education in science.

An important question for the working scientists themselves, who will train the next generation of scientists and who are often part of the public face of science, also presents itself. How much, and with what actual authority does popular science in fact speak for science in general? One possible source of conflict may well be over the type of disciplines that are considered sexy or important enough to get published (evolution, cosmology, medicine), against the quiet disciplines that we rarely hear about in a popular science context, such as almost all of chemistry, fluid dynamics or materials science. At least as far as can be judged from the individual authors of the books, it appears that string theory (Greene) has a different approach to simplicity than observational astronomy (Kirshner), though I had difficulties discerning a similar division in my interviews. (Also, as argued in chapter 5, the “outsider sciences” such as computer science may be more likely to accept Kuhn than other sciences – see also Potter’s 1984 analysis of discourse on Kuhn in a psychology conference, another “outsider science”).

But even setting disciplinary differences aside, there is still a danger that scientists can be easily alienated by the people who claim to be speaking for them. This is very visible already in some cases, notably Richard Dawkins who claims to speak for all scientists in his continuing polemic against religion. On a slightly

different note there seems to be some anger among scientists against popular forums such as the New Scientist magazine and some of the other typically PUS outreach programs such as science centres, museums or philosophically informed science education (Millar and Osborne for example needed to defend the “critical” science curriculum in a response article in the Guardian). It is certainly part of the strategies of many PUS and science education activities to portray science the way scientists see it, so my criticism is not directed at them on this point. Rather, I think my study shows that, at least when it concerns some particular aspects of the nature of science such as Occam’s razor, there may not be any definable scientific position that everybody would subscribe to at all, even if the perception exists that it is uncontroversial.

### *3.3. Lessons for philosophy*

In the above sections, and in chapter 6 on Occam’s razor I have argued that one very particular lesson surrounding Occam’s razor for philosophers is that they seem to take for granted a fact about how scientists think and philosophise around it. In this case, philosophers assume that scientists instinctively follow Occam’s razor. As this study has shown, this is actually true either with only a minority of scientists, or (taking the methodological and a fair few epistemological razors) only in ways that are unhelpful for much of the philosophical discussion.

This is a demonstration of a larger problem in the philosophy of science, as I outlined in the last point of the previous section. If a normative philosophy that tries to make injunctions about how science should be done deviates too much from how science is actually done, then it will cease to be credible. On the other hand if the philosophy merely describes what is going on, then it will make itself irrelevant for scientists. There is a much larger debate on this issue of naturalistic versus normative philosophy of science that I have reviewed briefly in chapter 2. This thesis throws up particular illustrations of this theme: One glaring example is that of Occam’s razor, but all the other themes I have discussed show a similar pattern.

Another lesson for philosophers is that through their social representations of philosophy, scientists will develop their own ways of talking about philosophical topics. If the formal relationship between scientists and philosophers deteriorates to the point that the philosophical opinions that scientists will inevitably have of their

subject are drawn almost exclusively from among the philosophical discourse of other scientists, then philosophy of science will be slightly redundant (though this can be debated as in the naturalism/normativism debate some philosophers claim that philosophy does not need to be heeded by scientists – see chapter 2). What is worse though is that in that case the philosophical discourse on philosophical topics and the scientific discourse on exactly the same topics will diverge so that the same terms will acquire different and confusing meanings. This can already be seen with reference to the different categories in which scientists think about philosophical topics in this study.

There is already a movement within current philosophy of science that seeks to address the issue of philosophising on actual scientific practice, with the set-up of the Society for the Philosophy of Science in Practice (Boon and Waelbers 2007, for the society's first conference proceedings). Although this approach addresses the relevance issue of philosophy of science, it still needs to monitor not only the way scientists practise science, but also the way science thinks about itself. Otherwise, while possibly the contents of philosophy of science can be made relevant to scientific practice, the way scientists understand that philosophy will be different to the way the philosophers understand it. Ultimately, whether philosophers like it or not, scientists will philosophise about science on their own terms, and if philosophy wants to participate in the discussion it must at least know what the philosophical issues are that scientists find relevant and interesting, but also how and in which categories they talk and understand philosophy, and when and under what circumstances philosophical topics become issues of identification and boundary work on top of their philosophical message. It is in identifying and keeping track of these issues that I believe my thesis has made a modest contribution to the philosophy of science itself.

#### **4. Concluding comments and future directions**

In this thesis I have shown how scientists, both in popular publications and in private conversations write and talk about the philosophy of science and particular philosophical concepts. I have shown how philosophy can fulfill several functions in scientific discourse: Philosophy can act as an authoritative boundary object which is used to communicate aspects of science to an audience which may not

know about the science in question or even the particular philosophy, but recognises that that particular point has philosophical authority behind it. Philosophy can also be used to draw a demarcating boundary around science or particular disciplines, where adherence to a philosophy can show that the scientist adheres to what he or she thinks is good scientific practice.

At the same time there is plenty of awareness of the substantive philosophical issues behind the famous philosophers and philosophical topics. The scientists were on the whole thoughtful about their activity and philosophised on it themselves on their own terms. I have found that the philosophical discourse on the substantive philosophical issues differs in many small ways from the way the philosophies are discussed when they are explicitly named. There is therefore a discrepancy that my study has found between the official philosophical terminology used by philosophers to discuss science and the way scientists think about science philosophically. I have used this fact to conclude that popular science books can often end up misrepresenting scientific thought on the nature of science in ways that many scientists would not necessarily recognise. It also suggests that philosophy is in danger of losing touch with scientific discourse on philosophy and therefore making itself redundant as far as its practical value is concerned.

In the absence of a comprehensive literature on scientists' representations of philosophy, this study is in many ways an explorative one, or as one of the interviewees described it, it is an hypothesis generating one. I have looked to cover many different philosophical issues as well as different disciplines and two different countries. One direction worth taking would be to widen it to include other sciences, maybe more marginally "scientific" ones such as psychology, engineering or even the social sciences. Similarly the study could be widened to cover more philosophical issues or go deeper into detail on the ones already covered.

Another option would be that the themes that I have picked up on, the representations of philosophy and of philosophy working as boundaries and identities between science and non-science and between disciplines, could be investigated deeper and more thoroughly. Now that I have identified the main themes of the usage of philosophy and the way that the scientists in my study have talked about philosophical topics – which philosophical topics they found

interesting in the first place – I feel that a further quantitative study can build and expand on the themes this thesis has identified and further test my interpretations of scientists' discourse, in an almost Popperian sense.

## **Appendix I: List of respondents**

### ***Pilot Study:***

1. Senior, Physics, Male, UK
2. Early Career, Physics, Male, UK
3. Early Career, Physics, Female, UK
4. Early Career, Physics, Male, UK

### ***UK Interviews***

5. Early Career, Physics, Male, UK
6. Senior, Chemistry, Male, UK
7. Senior, Biology, Male, UK
8. Senior, Biology, Female, UK
9. Senior, Physics, Male, UK
10. Early Career, Physics/Chemistry, Male, UK
11. Mid Career, Biology, Male, UK
12. Senior, Biology, Male, UK
13. Mid Career, Biology, Male, UK
14. Mid Career, Biology, Male, UK
15. Mid Career, Chemistry, Female, UK
16. Mid Career, Physics, Male, UK/Fr
17. Mid Career, Chemistry, Male, UK
18. Mid Career, Biology/Physics, Male, UK
19. Mid Career, Chemistry/Geology, Male, UK
20. Mid Career, Biology, Male, UK
21. Mid Career, Physics, Male, UK
22. Early Career, Physics, Female, UK
23. Early Career, Physics, Male, UK
24. Early Career, Biology, Female, UK
25. Early Career, Biology, Female, UK
26. Early Career, Biology, Female, UK/Fr
27. Mid Career, Biology, Female, UK



### ***Paris Interviews***

28. Early Career, Physics, Female, France
29. Early Career, Physics, Male, France
30. Mid Career, Physics, Female, France
31. Early Career, Physics, Female, France
32. Senior, Physics, Male, France/En
33. Senior, Physics, Male, France
34. Mid Career, Physics/Chemistry, Male, France
35. Senior, Biology/Physics, Female, France
36. Senior, Biology, Male, France
37. Senior, Biology, Male, France
38. Senior, Physics, Male, France
39. Senior, Biology, Male, France
40. Senior, Physics, Male, France

### ***Cancelled, but sent email:***

41. Mid Career, Physics, Male, UK

### **Notes:**

- In several cases the demarcation between the disciplines is not always clear-cut. For the interviewees where I discerned some significant identification with other disciplines, I have marked that here as well (respondents 10, 18, 19, 34, 35). The discipline mentioned first is always the one to which the respondent's department is affiliated.
- Regarding the nationality, in two cases the respondent was residing in the UK (16, 25), but came from a French-speaking country, while in another (32), the respondent worked in Paris but came from an English-speaking country. In a few cases the respondents came from neither English nor French speaking backgrounds, (Greece, Israel, Kenya, India and Austria). These will not be marked in the main text.
- In the classification of the respondents' career stage, I have striven to keep a rough distinction between early career (which includes three PhD students,

the rest are postdocs), mid career (by which I mean lecturers and senior lecturers or equivalent), and senior scientists (professors and equivalent). This remains a very rough distinction, as many respondents have followed unconventional career paths, and because French and UK research positions are often difficult to compare. Some people remain postdocs for a long time, while others managed to walk straight into a lectureship. Finally, some have had other careers before entering academia, and are therefore “behind” in their career compared to their age. These caveats aside however, there still remains a very strong correlation between age and seniority within the sample.

- The last respondent (41), had to cancel the interview due to an accident, but nevertheless still sent a long email explaining his views on scientific method and how his department teaches it.

## Appendix II: List of the popular science books

Shortlisted 2004:

- Armand Marie Leroi 2003: *Mutants*\*
- Matt Ridley 2003: *Nature via Nurture*\*
- Bill Bryson 2003: *A Short History of Nearly Everything* (Winner)
- Francis Spufford 2003: *Backroom Boys*
- Andrew Brown 2003: *In the Beginning was the Worm*
- Nigel Calder 2005: *Magic Universe*

Shortlisted 2003:

- Mark Buchanan 2002: *Small World*\*
- Gerd Gigerenzer 2002: *Reckoning with Risk*\*
- Robert Kirshner 2002: *The extravagant Universe*\*
- Chris McManus 2002: *Right Hand, Left Hand* (Winner)\*
- Steven Pinker 2002: *The Blank Slate*\*
- Stephen Webb 2002: *Where is everybody?*\*

Shortlisted 2002:

- Stephen Hawking 2001: *The Universe in a Nutshell* (Winner)\*
- David Horrobin 2001: *The Madness of Adam and Eve*\*
- Robert Sapolsky 2001: *A Primate's Memoir*\*
- Martin Gorst 2002: *Aeons: The Search for the Beginning of Time*
- Michael White 2002: *Rivals*
- Hannah Holmes 2001: *The Secret Life of Dust*

Shortlisted 2001:

- Steve Grand 2003: *Creation: Life and how to make it\**
- Lewis Wolpert 1999: *Malignant Sadness\**
- Mark Ridley 2000: *Mendel's Demon\**
- Robert Kunzig 2000: *Mapping the Deep - The Extraordinary Story of Ocean Science* (Winner)
- Paul Strathern 2000: *Mendeleyev's Dream - The Quest for the Elements*
- George Johnson 2000: *Strange Beauty - Murray Gell-Mann and the Revolution in Twentieth Century Physics*

Shortlisted 2000:

- Thomas Dormandy 1999: *The White Death\**
- Brian Greene 2000: *The Elegant Universe* (Winner)\*
- John Naughton 1999: *A brief History of the Future\**
- Christopher Wills 1998: *Children of Prometheus\**
- Jonathan Weiner 2000: *Time, Love, Memory*

Shortlisted 1999:

- Robert Weinberg 1998: *One renegade cell\**
- Edward O Wilson 1998: *Consilience\**
- Steven Pinker 1998: *How the Mind Works\**
- Paul Hoffman 1998: *The Man who Loved Only Numbers* (Winner)
- Sylvia Nasar 1998: *A Beautiful Mind*
- Rita Carter 1998: *Mapping the Mind*

Shortlisted 1998:

- David Deutsch 1997: *The Fabric of Reality\**

- Jared Diamond 1997: *Guns, Germs and Steel* (Winner)\*
- Richard Fortey 2000: *Life: An unauthorised Biography*\*
- Ernst Mayr 1997: *This is Biology*\*
- Simon Singh 1997: *Fermat's Last Theorem*
- Lawrence Wright 1997: *Twins*

Shortlisted 1997:

- Richard Dawkins 1997: *Climbing Mount Improbable*\*
- Steve Jones 1997: *In the blood: God, Genes and Destiny*\*
- Matt Ridley 1996: *The origins of virtue*\*
- Alan Walker and Pat Shipman 1996: *The Wisdom of Bones: In Search of Human Origins* (Winner)
- George Johnson 1996: *Fire in the Mind*
- Dava Sobel 1995: *Longitude*

Shortlisted 1996:

- Richard Dawkins 1995: *River out of Eden*\*
- Paul Davies 1995: *About time: Einstein's unfinished revolution*\*
- Arno Karlen 1995: *Plague's Progress* (Winner)
- Stephen Budiansky 1995: *Nature's Keepers*
- Ian Stewart 1995: *Nature's Numbers*
- John Carey (ed) 1995: *The Faber Book of Science*

**Notes:** At the time when the sample was constructed, there was no publicly available list of the prize shortlist, so I am indebted to the Royal Society press office for providing me with a list. It can now be viewed at the website of the Royal Society (Royal Society 2007).

The books selected for the sample are marked with an asterisk, see chapter 1, sect. 2.2 for the criteria used to decide which books to include. For a full list see Royal Society 2007. Some books show a publishing date after the year of the prize; this is due to me using a paperback or second edition.

### Appendix III: Table of the popular science books ordered by subject

|                          | Historical    | Technical        | Philosophical |
|--------------------------|---------------|------------------|---------------|
| <b>Physics</b>           |               |                  |               |
| Cosmology                |               | Greene 2000      | Davies 1995   |
|                          |               | Hawking 2001     |               |
|                          |               | Kirschner 2002   |               |
| Quantum physics          |               |                  | Deutsch 1997  |
| Networks                 |               | Buchanan 2002    |               |
| <b>Life Sciences</b>     |               |                  |               |
| General Biology          |               |                  | Mayr 1997     |
|                          |               |                  | Wilson 1998   |
| Medical                  |               |                  |               |
| Physical                 | Dormandy 1999 |                  |               |
|                          | Weinberg 1998 |                  |               |
|                          |               | Gigerenzer 2002  |               |
| Mental                   |               | Wolpert 1999     |               |
|                          |               | Horrobin 2001    |               |
| Genetics/evolution       |               |                  |               |
|                          |               | Dawkins 1995     |               |
|                          |               | Dawkins 1997     |               |
|                          |               | Jones 1997       |               |
|                          |               | Fortey 2000      |               |
|                          |               | Leroi 2003       |               |
|                          |               |                  | Pinker 1998   |
|                          |               | Pinker 2002      |               |
|                          |               | Mark Ridley 2000 |               |
|                          |               | Matt Ridley 1996 |               |
|                          |               | Matt Ridley 2003 |               |
|                          |               | Wills 1998       |               |
| Autobiographical         | Sapolsky 2001 |                  |               |
|                          |               |                  |               |
| <b>Computer Science</b>  |               | Grand 2003       |               |
|                          | Naughton 1999 |                  |               |
| <b>Interdisciplinary</b> |               |                  |               |
| Focus on life sci.       |               | Diamond 1997     |               |
|                          |               | McManus 2002     |               |
| Focus on physical sci.   |               | Webb 2002        |               |

Notes: Subject boundaries were in many cases quite fuzzy; see the overview in chapter 1. The books on evolution and genetics tended to focus on one of the two areas but in general were describing the two subjects together. No book on evolution would not feature a substantive chapter on genetics, and vice versa. I have therefore found it quite hard and split them apart.

I have also provided a very rough split between books that I felt had a substantial interest in philosophical matters, and books that stayed mostly within the technical limits of the science it describes. I have also added some books into a historical column: these are works that are mainly concerned with the history of the subject, or in one case, the personal history of the scientist. These splits are of course also very fuzzy: As philosophical I have included the books that are as much about popular philosophy of science as they are about science itself, such as Mayr and Deutsch. Some other books such as Pinker 1998 or Davies 1995 are on scientific topics that are themselves very philosophical: the nature of consciousness and time respectively. As such they feature plenty of particular philosophical discussion, but not on the more general philosophy of science that interests me in the thesis.



## Appendix IV: Initial letter

HAUKE RIESCH  
Dept. of Science and Technology  
Studies  
<http://www.ucl.ac.uk/sts>  
University College London  
Gower Street  
London WC1E 6BT

Dear Dr X

I am currently working on a PhD project sponsored by the Economic and Social Research Council at the Science and Technology Studies Department at UCL. I am looking at how scientific method is thought about by practising scientists. I am basing my study on a series of qualitative interviews, and am therefore keen to speak to a selection of scientists, such as yourself, from research intensive university departments.

In the interview I hope to cover issues such as how you would explain scientific method, and even whether you think such considerations are valuable at all, to either yourself as a practising scientist or for the general understanding of science by the public.

My motivation for this research is that many studies in science education and popular science communication suggest that teaching scientific method is essential, but otherwise take little account of what scientists themselves think about scientific method or its use in science teaching and popularisation.

Ideally, I would like to be able to talk to you for about an hour, and, with your agreement, to tape the interview. The interview will be held in a non-attributable way and I would be happy to provide you with some further information about my research beforehand, and to supply you with a copy of the transcript following the meeting.

I will contact you or your secretary in about two weeks' time to see if it is possible to arrange an interview.

Yours sincerely,

Hauke Riesch

## Appendix V: Initial letter (French)

Mr HAUKE RIESCH  
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University College London  
Gower Street  
London WC1E 6BT  
[h.riesch@ucl.ac.uk](mailto:h.riesch@ucl.ac.uk)

Dr X,

Je travaille actuellement sur un doctorat (PhD) sponsorisé par le Conseil de Recherche Economique et Sociale (ESRC) à l'université de Londres (University College London).

Je cherche à comprendre comment la nature de la science et la méthode scientifique sont perçues par les scientifiques. Je base mes études sur une série d'entretien qualitatif et aimerais ajouter quelques scientifiques français basés en France à mon étude des scientifiques Anglais.

Je vous écris dans l'espoir que vous, ou certains des vos collègues acceptiez de me rencontrer pour un entretien et je souhaiterais couvrir des sujets tels que comment expliquez vous la méthode scientifique et quelle est la distinction entre la science et les sujets que vous ne considérez pas comme science.

Ma motivation pour cette recherche est que de nombreuses études en éducation de la science et communication de la science suggèrent que l'enseignement de la méthode scientifique est essentielle mais n'accordent que peu d'intérêt à ce que les scientifiques en pensent.

Idéalement, j'aimerais avoir la possibilité de vous parler ainsi qu'à certain de vos collègues dans des sessions individuelles qui ne devraient pas plus d'une heure de votre temps et qui, avec l'accord du participant, seraient enregistrées et retranscrites.

Je serais à Paris du 11 au 22 Sept (pour une visite à l'université Paris VII) et serais très reconnaissant s'il était possible d'organiser quelques entretiens avec vous et vos collègues. Je ferais de mon mieux pour mener ces entretiens en français mais serais très heureux aussi si vous pouviez les faire en anglais. Ces entretiens seront anonymes et je serais ravie de vous faire parvenir de plus amples informations concernant ma recherche, et après l'entretien, de vous donner une copie de la transcription.

Je me permettrai de vous contacter dans les prochaines jours pour discuter avec vous de la possibilité d'une visite dans votre laboratoire.

Je vous prie d'agréer l'expression de mes salutations distinguées,

Hauke Riesch

## Appendix VI: Interview schedule

- Introductions, offer to give quick resume of my research
- Tell me a bit about your own research, how do you see your professional identity and that of your subject?
- What do you think is science? If the answer is something like “science is following the scientific method”, then what is the scientific method? What distinguishes science from what you think is definitely not science?
- How did you come to that opinion? Did they teach scientific method at your university or graduate school or was it through your own reading and/or introspection?
- How do you teach these topics yourself to your students (or if not, how would you teach them)?
- Have you heard of Karl Popper? If so, what do you think about his philosophy? If not, give a description of falsificationism. What do you think about it?
- Have you heard of Thomas Kuhn? Describe Kuhn’s philosophy if not. What are your thoughts about it?
- What do you think about reductionism (describe reductionism if term is unfamiliar). Are there fundamental laws in biology of chemistry?
- What do you think about the principle that in science you should always go for the simpler hypothesis? Is a simpler hypothesis more likely to be true?

**Note:** The above are prompts which were used to guide the interview. The schedule was usually followed roughly as indicated, although the precise question and follow-up questions varied, as they often fitted into the natural flow of the conversation. The last four questions, concentrating on the substantive philosophical issues which interested me most, were asked in different orders, depending on the previous conversation, as it often seemed natural to ask say the reductionism or Occam’s razor question first.

## Appendix VII: Coding frame, interviews

### Key:

#### Primary code

*Secondary code*

Tertiary code no. of occurrences

### 1: About the scientist: disciplinary identity, research etc.

#### *1.1: Biography*

Biography 44

Research 38

Typical day 16

#### *1.2: Identity*

Identity 35

Disciplines and interdisciplinarity 24

Theorist 4

Experimentalist 9

Experimentalist/theorist 21

Party/airplane response 3

Party response: discipline 20

Party response: scientist/lecturer 5

Party response: specific 12

### 2: Education on philosophy/scientific method

#### *2.0 General*

Education received on scientific method 41

#### *2.1 Tacit knowledge*

Following scientific method instinctively 3

Learning method from supervisor 18

Learning scientific method by example, doing 27

Idiosyncratic learning of method 1

#### *2.2 Learning philosophy of science*

Lectures attended in philosophy 14

Read philosophy him/herself 13

Teaches some philosophy 1

Teaching scientific method 29

Never had philosophy UG 11

No formal philosophy teaching 16

Philosophy offered but optional 8

### 3: What is and isn't science

#### *3.0: General comments on what is science/scientific method*

What is and isn't science 56

What is science is a difficult question 10  
Continuum between science and non-science 1  
Conventional descriptions of scientific method may be wrong 1  
Difference between industry and academic science 1  
Needs to think before committing 3  
Science is a tradition of thinking rather than a method 2 (3)  
Science is an approach/method 11 (3)

### *3.1 Hypothetico-deductivism (see also 4.2)*

Emphasis on hypothetico deductivism may be harmful 2  
Science is falsificationism 10 (5)  
Science is hypothetico-deductive 35 (14)  
Science is not HD 2  
Science is testability 12 (1)  
Science predicts things 10 (6)  
Science is verification 10 (1)  
Science is the elimination of hypotheses (1)  
Science done by machines 1

### *3.2 Science explains the world and it must be consistent*

Science aims at explaining facts 9 (2)  
Science is consistency across the disciplines/approaches 8 (7)  
Science is pulling different strands together 3 (2)  
Science is there to understand the world 16  
Unity of science 1  
Science is the pursuit of knowledge 2 (1)  
Science is the study of the world (1)

### *3.3 Why we do science*

Science is creative 4  
Science is curiosity driven 3  
Science is exciting/beautiful 3 (1)  
Science is like gambling 1  
Science is there to make applications 3  
Science is unpredictable 4  
Science is useful 6

### *3.4 Science is common sense*

Science is common sense 1  
Science is intuitive 4  
Science is not common sense 4

### *3.5 Science is impartial etc (see also 5.2 & 5.3)*

Science is honest (1)  
Science is not belief/religion 7  
Science is objective/impartial 13 (5)  
Science is open minded 7 (1)  
Science is rigorous/demanding 14 (4)  
Science is sceptical 1  
Peer review/self policing in science 3

Science is asking questions 2 (1)  
Science is rational 2

### *3.6 Science is replicable*

Science is not always repeatable 2  
Science is replicability 14 (5)  
Experiments are not always repeatable 2

### *3.7 Differences in method*

Difference of opinions/methods in the disciplines 39 (1)  
Scientific method idiosyncratic 3 (1)  
What is science changes over time 1

### *3.8 Science is inductive*

Science is a collection of facts 6  
Science is experimental/observational 11 (9)  
Science is generalisation 1 (4)  
Science is not a collection of facts 3  
Science is uniformity of nature 5 (1)  
Bayesianism 1

### *3.9 Science is more messy (see also 5.2 & 5.3)*

Science is actually more messy 18 (7)  
People are biased towards their ideas/pet theories 7  
Science is social 6 (1)  
Science is statistical 1  
Science is interpretation 2  
Uncertainty/error in experiments 4  
You can be too precise 1  
You have to accept some things on authority 1  
You sometimes have to take leaps of faith 1  
Truth will out! 4  
Scientists who understand method get promoted 3  
Publishing makes something true! 2

### *3.10 Science never really proves anything (realism) (see also 8.2)*

Science aims at truth/progress 5 (3)  
Science does not strictly prove anything 11 (1)  
Science has overwhelming evidence 2  
Science does not have definite answers 6 (4)  
Science is definite or almost definite answers 9 (1)  
Science is not a search for truth 2  
Science is weighing up evidence 2  
Science won't find out everything 1  
Science is proven 8  
Science is social construction 5  
Science is working so far (1)

### *3.11 Pseudosciences*

Astrology 11

Homeopathy 8  
Intelligent design 1  
Creationism 11  
Pseudoscience 1

### *3.12 Science is quantifiable*

Science is measuring 2  
Science is quantifiable 4 (1)

### *3.13 Other*

Controlled experiments 1  
Science answers how, not why questions 2  
Science is discovering new things 2 (2)  
Science is logical explanation 3  
Science is modelling 4  
Science is not reductionist 1  
Science is theoretical (2)  
Uncomfortable with concept of theories 2  
Controlled experiments 1  
Determinism 1  
Fitting evidence to suit theory 1  
Is theory or evidence more important? 2 (2)  
Null-hypothesis 5  
There are still open questions 1

## **4: Popper**

### *4.0 General*

Popper 45  
Popper and Kuhn not irreconcilable 3  
Popper well known with scientists 2  
Role of evidence/theories 10

### *4.1 Initial reactions*

Spontaneous mention of Popper 7  
Initial reaction to falsificationism 1  
Initial reaction to falsification ambivalent 15  
Initial reaction to falsification negative 10  
Initial reaction to falsificationism positive 15  
Has heard of falsificationism 7  
Has not heard of falsificationism 8  
Has heard of Popper 24  
Has not heard of Popper 7

### *4.2 Falsification/testability*

Evolution is not testable 1  
Falsification is not the whole story 20  
Falsification/philosophy in real science 49  
Testability in principle 2  
Testable hypotheses vital for funding 2



Role of confirmation 17  
There are crucial experiments 2  
Publishing negative results 4

#### *4.3 Underdetermination*

Underdetermination 20  
Underdetermination defeated by replication 1  
Underdetermination defeated by modifying hypothesis 5

### **5: Kuhn**

#### *5.0 Kuhn general*

Kuhn 43  
Kuhn loss 1

#### *5.1 Initial reactions*

Has heard of Kuhn 13  
Has not heard of Kuhn 18  
Initial reaction to Kuhn 7  
Initial reaction to Kuhn guarded 10  
Initial reaction to Kuhn negative 5  
Initial reaction to Kuhn positive 18  
Spontaneous mention of Kuhn 5  
Spontaneous mention of Paradigm 6

#### *5.2 Paradigms*

Paradigm, general 12  
Revolutions and paradigms 9  
Paradigm has negative connotations 1  
Paradigm picture analogy 5  
Paradigm shift doesn't mean discounting previous knowledge 8  
Paradigm shift means things become uninteresting 1

#### *5.3 Revolutions*

Revolutions happen, but very rarely 3  
Revolutions not the whole story 2  
There are complete revolutions 2  
There are no complete revolutions 5  
There are no revolutions in biology 4  
There are no revolutions in chemistry 2  
There are no revolutions in science 8

### **6: Occam's razor**

#### *6.0 OR general*

Simplicity 37  
Simplicity difficult to judge 13  
Simplicity generally accepted by scientists 6  
Simplicity is intuitive 10  
simplicity: different for biology and physics 1

simplicity: no clear cases where OR applicable 2  
Used to believe in simplicity 1

### *6.1 Initial reactions*

Initial reaction to simplicity guarded 11  
Initial reaction to simplicity negative 8  
Initial reaction to simplicity positive 13  
Initial reaction to simplicity, mentioning Occam 7  
Initial reaction to simplicity 4  
Spontaneous mention of Occam 6  
Spontaneous mention of simplicity 1

### *6.1 Epistemological/what is simple*

Epistemological razor 1  
Simple valuable as approximation 7  
Simplicity as explaining a lot with a little 2  
What is simple depends on background knowledge 3  
Simple possibly same as likely/probable 6  
simplicity: No. of parameters 1  
Simple not the same as likely 2

### *6.2 Ontological*

Simple is more likely to be true 7  
Simple is not more likely to be true 21  
World actually quite complicated 1  
World is unlikely to be simple 1  
Beauty 15  
Elegance 1

### *6.3 Methodological*

Simple is more useful/practical 29  
Simplicity not necessarily the easiest/most practical 1

### *6.4 Role of simplicity in science*

simplicity useful for dealing with ad hoc theories 3  
Simplicity has fooled us in the past 3  
Simplicity helpful in science 8  
Simplicity in everyday life 2  
Simplicity in GUT 6  
Simplicity may be harmful to science sometimes 6  
Simplicity unhelpful in science 3  
simplicity: No GUT possible 2  
simplicity: not always applicable 1  
Simplicity: OR invalid 2  
We should not be afraid of complexity 1

### *6.5 UFO question (can we use OR to explain UFO sightings?)*

UFO response: OR doesn't prove anything 3  
UFO response: burden of proof 1  
UFO: feasibility/plausibility 4

UFO: other things are not equal 2

## **7: Reductionism**

### *7.0 Reductionism general*

Reductionism 34

Holism 4

Reductionism and simplicity 10

Ambivalent towards reductionism 2

Positive towards reductionism 3

Used to be against reductionism 1

Evolution response 3

### *7.1 Initial reactions*

Has not heard of reductionism 11

Has heard of concept of reductionism 20

Initial reaction to reductionism 8

Initial reaction to reductionism guarded 15

Initial reaction to reductionism negative 6

Initial reaction to reductionism positive 9

Spontaneous mention of reductionism 1

### *7.2 Explanations of reductionism*

Own definition of reductionism 15

All laws obey physics 8

Emergent phenomena 4

Physics has no exclusive right to fundamentality 1

Reduction: all reducible to fundamental laws 9

Reduction: using languages of other sciences 1

Explanation 9

Reductionism in Rose's methodological sense 1

Reductionism spectrum 3

Reductionism: complexity/rare events 1

Systems biology 3

Reductionism: different levels of explanation 17

Reductionism: fundamental laws of biology 11

### *7.3 Usefulness of reductionism*

Reductionism fine in principle only 8

Reductionism as an insult 1

Reductionism not really helpful 6

Reductionism not really important for actual science 13

Some things can't be explained by reductionism 3

## **8: Other topics**

### *8.1 French philosophy*

French 20

Bachelard, spontaneous mention 2

Jacques Merleau-Ponty (not the famous M-P!) 1

Jacques Monod 1

## 8.2 Realism

Realism 41 (see also 3.9 and 3.10)

## 8.3 Role of philosophy/other comments on philosophy

Greek philosophy of science 3

Negative impression of philosophy 2

Philosopher doesn't understand science 2

Philosophers concentrate on physics 5

Philosophy general 20

Books on philosophy (1)

Philosophy is important 23

Uses of philosophy 26

What is philosophy? 1

You can defend anything with philosophy 1

Scientists are philosophically naive 6

Scientists ignore their own history 1

## 8.4 Named philosophers (see also 8.1)

Daniel Dennett 1

Francisco Ayala 1

Mach 1

Michael Ruse 1

Poincare 1

Polanyi 1

Russell 3

Wittgenstein 3

## 8.5 Popular science

Popular science 24

At who is popular science aimed? 1

## 8.6 Sociology

Social science 10

Sociology of science 9

## 8.7 Other

Gödel's theorem 1

Irrational beliefs 3

MMR vaccine 1

Role of postdocs 1

Serendipity 1 (1)

Research ethics 8

Publication pressure 8

## Notes:

- This coding frame applies to the interviews coded on Atlas.ti, as the books had been previously coded on NVivo. After the coding of the interviews I have reread the relevant book passages and coded them in Atlas.ti with the categories from the

interviews (in parentheses), though this only for the purposes of the “what is science” chapter. I was unable to carry over the original coding from the books on NVivo due to the corruption of my backup file. Fortunately by that time the analyses of the popular science books had already essentially been written. In general the coding for the books was much less dense and featured some particular philosophical points and issues that were discarded for the follow-up in the interviews (most of those I flagged up again when I recoded the books more superficially on Atlas). The codes occurring most often were Popper, Kuhn, Occam and reductionism which were further subdivided into comments supporting, rejecting or being neutral towards that philosophy/philosopher. Other codes were on these topics without the specific mention of a particular philosophy/philosopher, i.e. falsificationism, revolutions, simplicity, etc. Next to that there were codes for all the other philosophers (not just of science) that were mentioned in the books, such as David Hull, Dennett, Smart, Dingle, Nagel, Comte, “the logical positivists” and Feyerabend – only Popper, Kuhn and Occam however were mentioned with any frequency.

- A lot tertiary codes are relevant to several secondary and primary parents, especially since the Popper/Kuhn and "what is science" sections overlap significantly. Major overlaps are pointed out.



## References

- Abd-El-Khalick, Fouad and Norman G. Lederman. 2000a. "Improving Science Teachers' Conceptions of Nature of Science: a Critical Review of the Literature." *Science Education* 22(7):665-701.
- Abd-El-Khalick, Fouad and Norman G. Lederman. 2000b. "The Influence of History of Science Courses on Students' Views of Nature of Science." *Journal of Research in Science Teaching* 37(10):1057-95.
- Adorno, Theodor W., Hans Albert, Ralf Dahrendorf, Jürgen Habermas, Harald Pilot, and Karl Popper. 1976. *The Positivist Dispute in German Sociology*. English ed. London: Heinemann.
- Aguirre, Jose M., Sharon M. Haggerty, and Linder Cedric J. 1990. "Student-Teachers' Conceptions of Science, Teaching and Learning: a Case Study in Preservice Science Education." *International Journal of Science Education* 12(4):381-90.
- Alters, Brian J. 1997. "Whose Nature of Science?" *Journal of Research in Science Teaching* 34:39-55.
- Andersen, Hanne. 2001. "The History of Reductionism Versus Holistic Approaches to Scientific Research." *Endeavour* 25(4):153-56.
- Anderson, Don . 2002. "Occam's Razor: Simplicity, Complexity, and Global Geodynamics." *Proceedings of the American Philosophical Society* 146(1):56-76.
- Andler, Daniel and Saint-Sernin B. Fagot-Largeault. 2002. "Philosophie Des Sciences." Paris: Editions Galimard.
- Augoustinos, Martha. 2001. "Social Categorization: Towards Theoretical Integration." Pp. 201-16 in *Representations of the Social: Bridging Theoretical Traditions*, editors Kay Deaux and Phila Philogene. Oxford: Blackwell.
- Ayer, Alfred J. 2001. *Language, Truth and Logic*. London: Penguin.
- Bachelard, Gaston. 2002. *The Formation of the Scientific Mind*. Manchester: Clinamen Press.
- Bachelard, Gaston. 2003. *Le Nouvel Esprit Scientifique*. Paris: Presses Universitaires de France.
- Bacon, Francis. 2000. *The New Organon*. editors Lisa Jardine and Michael Silverthorne. Cambridge: Cambridge University Press.
- Bailer-Jones, Daniela M. 2003. "Scientists' Thoughts on Scientific Models." *Perspectives on Science* 10:275-301.
- Baker, Alan. 2004. "Simplicity." *The Stanford Encyclopedia of Philosophy (Winter 2004 Edition)*, editor Edward N. Zalta. <http://plato.stanford.edu/archives/win2004/entries/simplicity/>.

- Balmer, Brian. 1994. "Gene Mapping and Policy-Making: Australia and the Human Genome Project." *Prometheus* 12(1):3-18.
- Bar-Tal, Daniel. 1998. "Group Beliefs As an Expression of Social Identity." *Social Identity: International Perspectives*, Editors Stephen Worchel, J F. Morales, D Páez, and J Deschamps. London: Sage.
- Barnes, Barry, David Bloor, and John Henry. 1996. *Scientific Knowledge: A Sociological Analysis*. London: Athlone Press.
- Bauer, Henry H. 1994. *Scientific Literacy and the Myth of the Scientific Method*. Chicago: University of Illinois Press.
- Bauer, Martin, Nicholas Allum, and Steve Miller. 2007. "What Can We Learn From 25 Years of PUS Survey Research? Liberating and Expanding the Agenda." *Public Understanding of Science* 16:79-95.
- Bauer, Martin and Massimiano Bucchi, editors. 2007. *Journalism, Science and Society*. London: Routledge.
- Bauer, Martin and George Gaskell. 1999. "Towards a Paradigm of Research on Social Representations." *Journal for the Theory of Social Behaviour* 29(2):163-86.
- Bauer, Martin and George Gaskell. editors. 2000. *Qualitative Researching With Text, Image and Sound*. London: Sage.
- Bauer, Martin, George Gaskell, and Nicholas Allum. 2000. "Quality, Quantity and Knowledge Interests." *Qualitative Researching With Text, Image and Sound*, Editors Martin Bauer and George Gaskell. London: Sage.
- Bauer, Martin and Ingrid Schoon. 1993. "Mapping Variety in Public Understanding of Science." *Public Understanding of Science* 2:141-55.
- Billig, Michael. 1987. *Arguing and Thinking: a Rhetorical Approach to Social Psychology*. Cambridge: Cambridge University Press.
- Billig, Michael and Henri Tajfel. 1973. "Social Categorization and Similarity in Intergroup Behaviour." *European Journal of Social Psychology* 3:27-52.
- Bird, Alexander. 1998. *Philosophy of science*. Montreal: McGill-Queen's University Press
- Bishop, Ronald. 1999. "From Behind the Walls: Boundary Work by News Organizations in Their Coverage of Princess Diana's Death." *Journal of Communication Inquiry* 23(1):90-112.
- Bitbol, Michel and Jean Gayon, editors. 2006. *L'Épistémologie Française, 1830-1970*. Paris: PUF.
- Bloor, David . 1976. *Knowledge and Social Imagery*. Chicago: The University of Chicago Press.
- Boon, Mieke and Katinka Waelbers, editors. 2007. *Proceedings of the First Biannual Conference SPSP 2007*. Enschede: University of Twente.
- Bouilloud, Jean-Philippe. 2003. "The Reception of the Sokal Affair in France – "Pomo" Hunting or Intellectual McCarthyism?" *Philosophy of the Social Sciences* 33(1):122-37.

- Bowler, Peter. 2000. "Philosophy, Instinct, Intuition: What Motivates the Scientist in Search of a Theory?" *Biology and Philosophy* 15:93-101.
- Brampton, C K. 1964. "Nominalism and the Law of Parsimony." *The Modern Schoolman* 41:273-81.
- Broks, Peter. 2006. *Understanding Popular Science*. Maidenhead: Open University Press.
- Brown, Andrew. 2003. *In the Beginning was the Worm*. London: Simon & Schuster
- Brown, James R. 2001. *Who Rules in Science? An Opinionated Guide to the Science Wars*. Cambridge, MA: Harvard University Press.
- Brown, Rupert. 2000. "Social Identity Theory: Past Achievements, Current Problems and Future Challenges." *European Journal of Social Psychology* 30(6):745-78.
- Brown, Theodore L. 2003. *Making Truth: Metaphor in Science*. Urbana: University of Illinois Press.
- Brush, Stephen G. 1989. "Prediction and Theory Evaluation: The Case of Light Bending." *Science* 246:1124-29.
- Bryson, Bill. 2003. *A Short History of Nearly Everything*. London: Doubleday.
- Budiansky, Stephen. 1995. *Nature's Keepers: The New Science of Nature Management*. London: Weidenfeld & Nicolson
- Buchanan, Mark. 2002. *Small World*. London: Orion Books.
- Bunge, Mario . 1996. "In Praise of Intolerance to Charlatanism in Academia." *The Flight From Science and Reason*, Editors Paul R. Gross, Norman Levitt, and Martin W. Lewis. New York: The New York Academy of Sciences.
- Calder, Nigel. 2005. *Magic Universe*. Oxford: Oxford University Press
- Callebaut, Werner. 1993. "Taking the Naturalistic Turn or How Philosophy of Science Is Done." Chicago: The University of Chicago Press.
- Canguilhem, Georges. 2000. "A Vital Rationalist: Selected Writings From Goerges Canguilhem." New York: Zone Books.
- Carey, John, Editor. 1995. *The Faber Book of Science*. London: Faber & Faber
- Carter, Rita. 1998. *Mapping the Mind*. London: Weidenfeld & Nicolson
- Cassidy, Angela. 2003. *Of Academics, Publishers and Journalists: Popular Evolutionary Psychology in the UK*. Unpublished PhD thesis, University of Edinburgh.
- Cassidy, Angela. 2005. "Popular Evolutionary Psychology in the UK: an Unusual Case of Science in the Media?" *Public Understanding of Science* 14(2):115-41.
- Cassidy, Angela. 2006. "Evolutionary Psychology As Public Science and Boundary Work." *Public Understanding of Science* 15(2):175-205.
- Castelão-Lawless. 2004. "Kuhn's Missed Opportunity and the Multifaceted Life of Bachelard:



- Mythical, Institutional, Historical, Philosophical, Literary, Scientific." *Studies in the History and Philosophy of Science* 35:873-81.
- Ceccarelli, Leah. 2001. *Shaping Science With Rhetoric: the Cases of Dobzhansky, Schrödinger, and Wilson*. Chicago: University of Chicago Press.
- Chagnon, Napoleon. 1974. *Studying the Yanomamö*. London: Holt, Rinehart and Winston.
- Chalmers, Alan. 1982. *What Is This Thing Called Science? An Assessment of the Nature and Status of Science and Its Methods*. 2nd ed. Milton Keynes: Open University Press.
- Chang, Hasok. 1999. "History and Philosophy of Science As a Continuation of Science by Other Means." *Science and Education* 8:413-25.
- Chang, Hasok. 2004. *Inventing Temperature*. Oxford: Oxford University Press.
- Charney, Davida. 2003. "Lone Geniuses in Popular Science - the Devaluation of Scientific Consensus." *Written Communication* 20(3):215-41.
- CNRS. 2008. "Annuaire des laboratoires" [Web Page]. Accessed 19 Aug 2008. Available at <http://web-ast.dsi.cnrs.fr/l3c/owa/annuaire.recherche/index.html>.
- Collins, Harry. 2001. "Tacit Knowledge, Trust and the Q of Sapphire." *Social Studies of Science* 31(1):71-85.
- Crane, Diana . 1972. *Invisible Colleges: Diffusion of Knowledge in Scientific Communities*. Chicago: University of Chicago Press.
- Crease, Robert. 2001. "What's Your Philosophy?" *Physics World* 14(10).
- Crease, Robert. 2002. "This Is Your Philosophy." *Physics World* 15(4):15-17.
- Curd, M and J A. Cover, Editors. 1998. *Philosophy of Science: the Central Issues*. London: W. W. Norton.
- Davies, Paul. 1995. *About Time: Einstein's Unfinished Revolution*. London: Viking.
- Davies, Paul and Julian R. Brown, editors. 1986. *The Ghost in the Atom: A Discussion of the Mysteries of Quantum Physics*. Cambridge: Cambridge University Press.
- Davies, Sharon. 2003. "Obituary for David Horrobin: Summary of Rapid Responses." *British Medical Journal* 326(17 May):1089.
- Davson-Galle, Peter. 2006. "Why Compulsory Science Education Should *Not* Include Philosophy of Science." *Science and Education* Online First.
- Dawkins, Richard. 1986. *The Blind Watchmaker*. London: Penguin.
- Dawkins, Richard. 1995. *River Out of Eden: a Darwinian View of Life*. London: Weidenfeld & Nicolson.
- Dawkins, Richard. 1997. *Climbing Mount Improbable*. London: Penguin.

- Dawkins, Richard. 2003. *A Devil's Chaplain*. London: Weidenfeld & Nicolson.
- Dawkins, Richard. 2006. *The Selfish Gene*. Oxford: Oxford University Press.
- Deutsch, David. 1997. *The Fabric of Reality*. London: Penguin.
- Diamond, Jared. Vintage. *Guns, Germs and Steel*. 1997: London.
- Doise, Willem. 1998. "Social Representations in Personal Identity." *Social Identity: International Perspectives*, editors Stephen Worchel, J. F. Morales, Dario Páez, and Jean-Claude Deschamps. London: Sage.
- Domingos, Pedro. 1999. "The Role of Occam's Razor in Knowledge Discovery." *Data Mining and Knowledge Discovery* 3(4):409-25.
- Donovan, Arthur, Larry Laudan, and Rachel Laudan, Editors. 1992. *Scrutinizing Science*. London: Johns Hopkins University Press.
- Dormandy, Thomas. 1999. *The White Death: a History of Tuberculosis*. London: Hambledon.
- Driver, Rosalind, John Leach, Robin Millar, and Phil Scott. 1996. *Young People's Images of Science*. Buckingham: Open University Press.
- Duhem, Pierre. 1991. *The Aim and Structure of Physical Theory*. translator Philip Wiener. Princeton: Princeton University Press.
- Dupré, John. 1983. "The Disunity of Science." *Mind* xcii :321-46.
- Durant, John. 1993. "What Is Scientific Literacy?" *Science and Culture in Europe*, editors John Durant and Jane Gregory. London: Science Museum.
- Duveen, Gerard. 2000. "Introduction: The Power of Ideas." *Social Representations*, Serge Moscovici. Cambridge: Polity.
- EAESP (European association of experimental social psychology of science). 2006. "Biography of Henri Tajfel" [Web Page]. Accessed 25 Jan 2008. Available at <http://www.eaesp.org/activities/own/awards/tajfel.htm>.
- Earman, John . 1992. *Bayes or Bust? A Critical Examination of Bayesian Confirmation Theory*. Cambridge, MA: MIT Press.
- Fahnestock, Jeanne. 1986. "Accommodating Science: The Rhetorical Life of Scientific Facts." *Written Communication* 3:275-96.
- . 1998. "Arguing in Different Forums: The Bering Crossover Controversy." *Science, Technology & Human Values* 1:26-42.
- Farmelo, Graham, Editor. 2002. *It Must Be Beautiful*. London: Granta.
- Farr, Robert. 1993. "Common Sense, Science and Social Representations." *Public Understanding of Science* 2:189-204.
- Feyerabend, Paul. 1993. *Against Method*. London: Verso.

- Feynman, Richard. 1965. *The Character of Physical Law*. Cambridge, MA: MIT Press.
- Fortey, Richard. 2000. *Life*. New York: Alfred A Knopf.
- Foucault, Michel. 1994. *The Birth of the Clinic: an Archaeology of Medical Perception*. New York: Vintage.
- Foucault, Michel. 2002. *Archaeology of Knowledge*. London : Routledge.
- Fuller, Steve. 1993. *Philosophy of Science and Its Discontents*. New York: The Guilford Press.
- Fuller, Steve. 2003. *Kuhn Vs Popper*. Cambridge: Icon.
- Gallagher, James J. 1990. "Prospective and Practicing Secondary School Science Teachers' Knowledge and Beliefs About the Philosophy of Science." *Science Education* 75(1):121-33 .
- Geertz, Clifford. 1973. *The Interpretation of Cultures: Selected Essays*. New York: Basic Books.
- Giaquinto, Marcus. 2002. *The Search for Certainty: a Philosophical Account of Foundations of Mathematics*. Oxford: Clarendon Press.
- Gieryn, Thomas F. 1983. "Boundary-Work and the Demarcation of Science From Non-Science: Strains and Interests in Professional Ideologies of Scientists." *American Sociological Review* 48(6):781-95.
- Gieryn, Thomas F. 1995. "Boundaries of Science." Pp. 393-443 in *Handbook of Science and Technology Studies*, Editors Sheila Jasanoff, Gerald E. Markle, James C. Petersen, and Trevor Pinch. London: Sage.
- Gieryn, Thomas F. 1999. *Cultural Boundaries of Science: Credibility on the Line*. Chicago : University of Chicago Press.
- Gigerenzer, Gerd. 2002. *Reckoning With Risk*. London: Penguin.
- Gilbert, G N. and Michael Mulkay. 1982. "Warranting Scientific Belief." *Social Studies of Science* 12(3):383-408.
- Gilbert, G N. and Michael Mulkay. 1984. *Opening Pandora's Box: a Sociological Analysis of Scientists' Discourse*. Cambridge: Cambridge University Press.
- Gillies, Donald. 1993. *Philosophy of Science in the Twentieth Century: Four Central Themes*. Oxford: Blackwell.
- Gillies, Donald. 2000. *Philosophical Theories of Probability*. London: Routledge.
- Gleick, James. 1988. *Chaos: Making a New Science* . London: Heinemann.
- Gorst, Martin. 2002. *Aeons: The Search for the Beginning of Time*. London: Fourth Estate
- Gould, Steven J. 1980. *The Panda's Thumb*. London: Penguin.
- Gould, Steven J. 1992. *The Mismeasure of Man*. London : Penguin.

- Gould, Steven J. 2003. *The Hedgehog, the Fox, and the Magister's Pox: Mending the Gap Between Science and the Humanities*. London: Jonathan Cape.
- Grafen, Alan and Mark Ridley, editors. 2006. *Richard Dawkins :How a Scientist Changed the Way We Think: Reflections by Scientists, Writers, and Philosophers*. Oxford: Oxford University Press.
- Grand, Steve. 2000. *Creation: Life and How to Make It*. London: Orion Books.
- Grand, Steve. 2004. [Web Page]. Accessed 29 Jan 2008. Available at <http://www.cyberlife-research.com/people/steve/>.
- Grand, Steve and Dave Cliff. 1998. "Creatures: Entertainment Software Agents With Artificial Life." *Autonomous Agents and Multi-Agent Systems* 1(1):39-57.
- Greene, Brian. 2000. *The Elegant Universe*. London: Random House.
- Gregory, Jane. 2003. "Popularization and Excommunication of Fred Hoyle's "Life-From-Space" Theory." *Public Understanding of Science* 12(1):25-46.
- Gregory, Jane and Steve Miller. 1998. *Science in Public*. London: Plenum.
- Gross, Paul R. and Norman Levitt. 1994. *Higher Superstition: The Academic Left and Its Quarrels With Science*. Baltimore: Johns Hopkins University Press.
- Gross, Paul R., Norman Levitt, and Martin W. Lewis, Editors. 1996. *The Flight From Science and Reason*. New York: New York Academy of Sciences.
- Gutting, Gary. 2001. *French Philosophy in the Twentieth Century*. Cambridge: Cambridge University Press.
- Gutting, Gary. editor. 2005. *Continental Philosophy of Science*. Malden, MA: Blackwell.
- Haack, Susan. 1996. "Concern for Truth : What It Means, Why It Matters." *The Flight From Science and Reason*, editors Paul R. Gross, Norman Levitt, and Martin W. Lewis. New York: New York Academy of Sciences.
- Hacking, Ian. 1999. *The Social Construction of What?* Cambridge, MA: Harvard University Press.
- Haldane, J. B. S. 1931. *The Philosophical Basis of Biology*. London: Hodder and Stoughton.
- Hamilton, William. 1853. *Discussions in Philosophy and Literature, Education and University Reform*. London: Longman, Brown, Green and Longman's.
- Hammersley, Martyn. 1996. "The Relationship Between Qualitative and Quantitative Research: Paradigm Loyalty Versus Methodological Eclecticism." *Handbook of Qualitative Research Methods for Psychology and the Social Sciences*, editor John Richardson . Leicester: The British Psychological Society.
- Harmon-Jones, Eddie and Judson Mills, editors. 1999. *Cognitive Dissonance: Progress on a Pivotal Theory in Social Psychology*. London: American Psychological Association.
- Hawking, Stephen. 1988. *A Brief History of Time: From the Big Bang to Black Holes*. London: Bantam.

- Hawking, Stephen. 2001. *The Universe in a Nutshell*. London: Random House.
- Heisenberg, Werner. 1969. *Der Teil Und Das Ganze: Gespräche Im Umkreis Der Atomphysik*. Munich: Piper.
- Heisenberg, Werner. 1989. *Physics and Philosophy*. London: Penguin.
- HERO. 2002. "RAE 2001" [Web Page]. Accessed 18 Jan 2008. Available at [www.hero.ac.uk/rae/](http://www.hero.ac.uk/rae/).
- Hoffman, Paul. 1998. *The Man who Loved Only Numbers*. London: Fourth Estate
- Hogg, Michael A. 2007. "Social Identity and the Group Context of Trust: Managing Risk and Building Trust Through Belonging." Pp. 51-72 in *Trust in Corporate Risk Management*, editors Michael Siegrist, Timothy C. Earle, and Heinz Gutscher. London: Earthscan.
- Hogg, Michael A. and Dominic Abrams. 1988. *Social Identifications*. London: Routledge.
- Holmes, Hannah. 2001. *The Secret Life of Dust*. Chichester: Wiley
- Holmquest, Anne. 1990. "The Rhetorical Strategy of Boundary-Work." *Argumentation* 4(3):235-58.
- Holton, Gerald. 1984. "Do Scientists Need a Philosophy?" *Times Literary Supplement*(Nov 2):1231-34.
- Horrobin, David. 2001. *The Madness of Adam and Eve*. London: Bantam Press.
- Howard, Judith A. 2000. "Social Psychology of Identities." *Annual Review of Sociology* 26:367-93.
- Howson, Colin and Peter Urbach. 1993. *Scientific Reasoning: the Bayesian Approach*. 2nd ed. Chicago: Open Court.
- Hoyningen-Huene, Paul. 1993. *Reconstructing Scientific Revolutions: Thomas S. Kuhn's Philosophy of Science*. Chicago: University of Chicago Press.
- Huddy, Leonie. 2001. "From Social to Political Identity: A Critical Examination of Social Identity Theory." *Political Psychology* 22(1):127-56.
- Hull, David, Peter Tessner, and Arthur Diamond. 1978. "Planck's Principle." *Science* 202(4369):717-23.
- Hume, David. 1999. *An Enquiry Concerning Human Understanding* Tom Beauchamp. Oxford: Oxford University Press.
- Jardine, Nick and Marina Frasca-Spada. 1997. "Splendours and Miseries of the Science Wars." *Studies in the History and Philosophy of Science* 28(2):219-35.
- Jeanneret, Yves. 1998. *L'Affaire Sokal Ou La Querelle Des Impostures*. Paris: Presses Universitaires de France.
- Johnson, George. 1996. *Fire in the Mind*. London: Viking
- Johnson, George. 2000. *Strange Beauty - Murray Gell-Mann and the Revolution in Twentieth Century Physics* London: Jonathan Cape

- Jones, Steve. 1997. *In the Blood: God, Genes and Destiny*. London: Flamingo.
- Jurdant, Badouin. 1993. "Popularization of Science As the Autobiography of Science." *Public Understanding of Science* 2:365-73.
- Jurdant, Badouin. editor. 1998. *Impostures Scientifiques*. Paris: Editions La Decouverte.
- Kant, Immanuel. 1993. *Critique of Pure Reason*. editor Vasilis Politis. London: J.M. Dent.
- Karlen, Arno. 1995. *Plague's Progress*. London: V. Gollancz
- Kelly, Kevin. 2004. "Justification As Truth-Finding Efficiency: How Ockham's Razor Works." *Minds and Machines* 14:485-505.
- Kirshner, Robert P. 2002. *The Extravagant Universe*. Princeton: Princeton University Press.
- Kitcher, Philip. 1983. *Abusing Science: The Case Against Creationism*. Milton Keynes: Open University Press.
- Knorr-Cetina, Karin. 1995. "Laboratory Studies: The Cultural Approach to the Study of Science." *Handbook of Science and Technology Studies*, Editors Sheila Jasanoff, Gerald E. Markle, James C. Petersen, and Trevor Pinch . London: Sage.
- Knorr-Cetina, Karin. 1999. *Epistemic cultures : how the sciences make knowledge*. Cambridge, MA: Harvard University Press
- Koertge, Noretta, editor. 1998. *A House Build on Sand*. Oxford: Oxford University Press.
- Koponen, Ismo T. 2006. "Models and Modelling in Physics Education: A Critical Re-Analysis of Philosophical Underpinnings and Suggestions for Revisions." *Science and Education* 16(7-8):751-73.
- Koponen, Ismo T. and Terhi Mäntylä. 2006. "Generative Role of Experiments in Physics and in Teaching Physics: A Suggestion for Epistemological Reconstruction." *Science and Education* 15(1):31-54.
- Kornblith, Hilary, editor. 1994. *Naturalizing Epistemology*. Cambridge, MA: MIT Press.
- Koulaidis, Vasilios and Jon Ogborn. 1989. "Philosophy of Science: an Empirical Study of Teachers' Views ." *International Journal of Science Education* 11(2):173-84.
- Krauss, Lawrence. 1995. *The Physics of Star Trek*. London: Harper Collins.
- Kuhn, Thomas S. 1962. *The Structure of Scientific Revolutions*. Third ed. Chicago: University of Chicago Press.
- Kunzig, Robert. 2000. *Mapping the Deep - The Extraordinary Story of Ocean Science* London: Sort Of Books
- Ladyman, James. 2001. *Understanding the philosophy of science*. London: Routledge
- Lakatos, Imre. 1970. "Falsification and the Methodology of Scientific Research Programmes." *Criticism and the Growth of Knowledge*, Editors Imre Lakatos and Alan Musgrave. Cambridge:

Cambridge University Press.

Lakatos, Imre. 1978. *The Methodology of Scientific Research Programmes*. Cambridge: Cambridge University Press.

Lakatos, Imre and Alan Musgrave, editors. 1970. *Criticism and the Growth of Knowledge*. Cambridge: Cambridge University Press.

Lambert, Kevin. 2006. "Fuller's Folly, Kuhnian Paradigms, and Intelligent Design." *Social Studies of Science* 36(6):835-42.

Lamont, Michele. 1992. *Money, Morals, and Manners: the Culture of the French and American Upper-Middle Class*. Chicago: University of Chicago Press.

Lamont, Michele. 2001. "Symbolic Boundaries" [Web Page]. Accessed 28 Jan 2008. Available at <http://falcon.jmu.edu/~brysonbp/symbound/papers2001/LamontEncyclo.html>.

Lamont, Michele and Virag Molnar. 2002. "The Study of Boundaries in the Social Sciences." *Annual Review of Sociology* 28:167-95.

Laudan, Larry. 1977. *Progress and Its Problems*. London: Routledge.

Laudan, Larry. 1998. "Commentary: Science at the Bar - Causes for Concern." Pp. 48-53 in *Philosophy of Science: the Central Issues*, M Curd and J A. Cover. New York: W. W. Norton & Company.

Lecourt, Dominique. 1999. "Rapport au ministre de L'Education nationale, de la Recherche et de la Technologie: L'enseignement de la philosophie des sciences" [Web Page]. Accessed 18 Jan 2008. Available at <http://pedagogie.ac-toulouse.fr/philosophie/ensei/rapportlecourt.htm>.

Lederman, Norman G. 1991. "Students' and Teachers' Conceptions of the Nature of Science: A Review of the Research." *Journal of Research in Science Teaching* 29(4):331-59.

Leroi, Armand M. 2003. *Mutants*. London: HarperCollins.

Levins, Richard and Richard Lewontin. 1985. *The Dialectical Biologist*. Cambridge, MA: Harvard University Press.

Lewenstein, Bruce V. 1995. "From Fax to Facts: Communication in the Cold Fusion Saga." *Social Studies of Science* 25:408-24.

Lewontin, Richard. 1993. *The Doctrine of DNA*. London: Penguin.

Lewontin, Richard. 2000. *It Ain't Necessarily So: the Dream of the Human Genome and Other Illusions*. London: Granta.

Luminet, Jean-Pierre. 2001. *L'Univers Chiffonné*. Paris: Gallimard.

Lynch, Michael. 2006. "From Ruse to Farce." *Social Studies of Science* 36(6):819-26.

Lyne, John and Henry Howe. 1990. "The Rhetoric of Expertise: E. O. Wilson and Sociobiology." *Quarterly Journal of Speech* 76(2):134-51.

- Masterman, Margaret. 1970. "The Nature of a Paradigm." Pp. 59-90 in *Criticism and the Growth of Knowledge*, editors Imre Lakatos and Alan Musgrave. Cambridge: Cambridge University Press.
- Matthewman, Steve and Douglas Hoey. 2006. "What Happened to Postmodernism?" *Sociology* 40(3):529-47.
- Matthews, Michael R. 1994. *Science Teaching. The Role of History and Philosophy of Science*. London: Routledge.
- Matthews, Michael R. 1998. "In Defense of Modest Goals When Teaching About the Nature of Science." *Journal of Research in Science Teaching* 35(2 ):161-74.
- Matthews, Michael R. 2003. "Thomas Kuhn's Impact on Science Education." *Science Education* 88:90-118.
- Matthews, Michael R. 2004. "Reappraising Positivism and Education: The Arguments of Philip Frank and Herbert Feigl." *Science and Education* 13:7-39.
- Maurer, Armand A. 1978. "Method in Ockham's Nominalism." *The Monist* 61:426-43.
- Mayr, Ernst . 1997. *This Is Biology*. Cambridge MA: Harvard University Press.
- McManus, Chris. 2002. *Right Hand, Left Hand: the Origins of Asymmetry in Brains, Bodies, Atoms and Cultures*. London: Weidenfeld & Nicolson.
- Medawar, Peter. 1979. *Advice to a Young Scientist* . London: Harper and Row.
- Medawar, Peter. 1996. *The Strange Case of the Spotted Mice*. Oxford: Oxford University Press.
- Mellor, Felicity. 2003. "Between Fact and Fiction: Demarcating Science From Non-Science in Popular Physics Books." *Social Studies of Science* 33(4):509-38.
- Merton, Robert K. 1973. *The Sociology of Science*. Chicago: The University of Chicago Press.
- Michael, Mike. 2002. "Comprehension, Apprehension, Prehension: Heterogeneity and the Public Understanding of Science." *Science, Technology & Human Values* 27(3):357-78.
- Miller, Arthur I. 1996. *Insights of Genius: Imagery and Creativity in Science and Art*. New York: Copernicus.
- Miller, Jon D. 1987. "Scientific Literacy in the United States." *Communicating Science to the Public*, Editors David Evered and Maeve O'Connor. Chichester: John Wiley & Sons.
- Miller, Jon D. 1993. "Theory and Measurement in the Public Understanding of Science: a Rejoinder to Bauer and Schoon." *Public Understanding of Science* 2:235-43.
- Miller, Steve. 2001. "Public Understanding of Science at the Crossroads." *Public Understanding of Science* 10:115-20.
- Monod, Jacques. 1974. *Chance and Necessity: an Essay on the Natural Philosophy of Modern Biology*. London: Fontana.
- Monod, Jacques. 2007. "Preface." *La Logique De La Découverte Scientifique*, Karl Popper. Paris:



Payot.

Moore, John A. 1993. *Science As a Way of Knowing*. Cambridge, MA: Harvard University Press.

Morange, Michel. 2007. "What History Tells Us XI: The Complex History of the Chemiosmotic Theory." *Journal of Biosciences* 32(7):1245-50.

Moscovici, Serge. 1993. "Toward a Social Psychology of Science." *Journal for the Theory of Social Behaviour* 23(4):21.

Moscovici, Serge. 2000. *Social Representations*. editor Gerard Duveen. Cambridge: Polity Press.

Moscovici, Serge. 2007. *Psychoanalysis: Its Image and Its Public*. London: Blackwell.

Mulkay, Michael and G N. Gilbert. 1981. "Putting Philosophy to Work: Karl Popper's Influence on Scientific Practice." *Philosophy of the Social Sciences* 11:389-407.

Mummendey, Amélie and Michael Wenzel. 1999. "Social Discrimination and Tolerance in Intergroup Relations: Reactions to Intergroup Difference." *Personality and Social Psychology Review* 3(2):158-74.

Myers, Greg . 2003. "Discourse Studies of Scientific Popularization: Questioning the Boundaries." *Discourse Studies* 5(2):265-79.

Myung, In J. and Mark A. Pitt. 1997. "Applying Occam's Razor in Modelling Cognition: A Bayesian Approach." *Psychonomic Bulletin & Review* 4(1):79-95.

Nagel, Ernest. 1961. *The Structure of Science*. London: Routledge.

Nasar, Sylvia. 1998. *A Beautiful Mind*. London: Faber & Faber

National Academy of Sciences. 1998. *Teaching About Evolution and the Nature of Science*. Washington DC: National Academies Press .

Naughton, John. 1999. *A Brief History of the Future*. London: Orion.

Nieman, Adam. 2000. *The Popularization of Physics: Boundaries of Authority and the Visual Culture of Science*. Unpublished PhD thesis, University of the West of England .

NSF. 2008. "Science and engineering indicators 2008" [Web Page]. Accessed 18 Jan 2008. Available at [www.nsf.gov/statistics/seind08/](http://www.nsf.gov/statistics/seind08/).

Ortony, Andrew, editor. 1993. *Metaphor and Thought*. 2nd ed. Cambridge: Cambridge University Press.

Osborne, Jonathan and Robin Millar. 2007. "There's No Cynicism About Our New Science Course." *The Guardian* March 7.

Osborne, Jonathan, Mary Ratcliffe, and Rick Duschl. 2003. "What 'Ideas-About-Science' Should Be Taught in School Science? A Delphi Study of the Expert Community." *Journal of Research in Science Teaching* 40(7):692-720.

Paul, Danette. 2004. "Spreading Chaos - The Role of Popularization in the Diffusion of Scientific

- Ideas." *Written Communication* 21(1):32-68.
- Pinker, Steven. 1998. *How the Mind Works*. London: Allen Lane.
- Pinker, Steven. 2002. *The Blank Slate*. London: Penguin.
- Poincaré, Henri. 2001. *The Value of Science*. New York: Modern Library.
- Polanyi, Michael. 1958. *Personal Knowledge*. London: Routledge.
- Polkinghorne, John. 2000. *Faith, Science and Understanding*. London: SPCK.
- Popper, Karl. 1992. *Conjectures and Refutations* . London: Routledge.
- Popper, Karl. 2002. *The Logic of Scientific Discovery*. London: Routledge.
- Potter, Jonathan. 1984. "Testability, Flexibility: Kuhnian Values in Scientists' Discourse Concerning Theory Choice." *Philosophy of the Social Sciences* 14:303-30.
- Potter, Jonathan and Margaret Wetherell. *Discourse and Social Psychology: Beyond Attitudes and Behaviour*. London: Sage.
- Pratchett, Terry, Ian Stewart, and Jack Cohen. 1999. *The Science of Discworld*. London: Ebury Press.
- Quine, Willard V. O. 1980. *From a Logical Point of View*. second ed. Cambridge, MA: Harvard University Press.
- Quine, Willard V. O. 1994. "Epistemology Naturalized." *Naturalizing Epistemology*, editor Hilary Kornblith. Cambridge, MA: MIT Press.
- Reisch, George. 2005. *How the Cold War Transformed Philosophy of Science*. Cambridge: Cambridge University Press.
- Rheinberger, Hans-Jörg. 2005. "Reassessing the Historical Epistemology of Georges Canguilhem." *Continental Philosophy of Science*, Gary Gutting. Malden, MA: Blackwell.
- Richardson , John, editor. 1996. *Handbook of Qualitative Research Methods for Psychology and the Social Sciences*. Leicester: The British Psychological Society.
- Richmond, Caroline. 2003. "David Horrobin." *British Medical Journal* 326(19 April):885.
- Ridley, Mark. 2000. *Mendel's Demon*. London: Orion Books.
- Ridley, Matt. 1996. *The Origins of Virtue*. London: Viking.
- Ridley, Matt. 1999. *Genome*. London: Fourth Estate.
- Ridley, Matt. 2003. *Nature Via Nurture*. London : Fourth Estate.
- Rodriguez-Fernandez, Jose L. 1999. "Ockham's Razor ." *Endeavour* 23(3):121-25.
- Rose, Steven. 1992. *The Making of Memory*. New York: Bantam.

Royal Society . 2008. "Royal Society prizes for Science Books" [Web Page]. Accessed 18 Jan 2008. Available at <http://www.royalsoc.ac.uk/sciencebooks>.

Ruse, Michael. 1989. "Sociobiology and Reductionism." *Reductionism and Systems Theory in the Life Sciences*, editors Paul Hoyningen-Huene and Franz Wuketits. Dordrecht: Kluwer.

Ruse, Michael. 1994. "Knowledge and Human Genetics: Some epistemological Questions. " *Genes and Human self-knowledge*, editors R. Weir, S. Lawrence and E. Fales. Iowa City: University of Iowa Press

Ruse, Michael. 1996. *Monad to Man: The Concept of Progress in Evolutionary Biology*. Cambridge, MA: Harvard University Press.

Ruse, Michael. 1998. "Creation-Science Is Not Science." Pp. 38-47 in *Philosophy of Science: the Central Issues*, M Curd and J A. Cover. New York: W. W. Norton & Company.

Russell, Bertrand. 2001. *The Problems of Philosophy*. Oxford: Oxford University Press.

Ryder, Jim, John Leach, and Rosalind Driver. 1999. "Undergraduate Science Students' Images of Science." *Journal of Research in Science Teaching* 36(2):201-19.

Sagan, Carl . 1996. *The Demon-Haunted World*. London: Headline.

Sankey, Howard. 1997. *Rationality, Relativism and Incommensurability*. Aldershot: Ashgate.

Sapolsky, Robert M. 2001. *A Primate's Memoir*. London: Random House.

Secord, Paul F. 1959. "Stereotyping and Favourableness in the Perception of Negro Faces." *Journal of Abnormal and Social Psychology* 59:309-15 .

Segerstråle, Ullica, editor. 2000. *Beyond the Science Wars: The Missing Discourse About Science and Society*. Albany: SUNY Press.

Segerstråle, Ullica. 2000. *Defenders of the Truth*. Oxford: Oxford University Press.

Shermer, Michael and Frank Sulloway. 2004. "The Grand Old Man of Evolution: An Interview With Evolutionary Biologist." *Skeptic* 8(1):76-82.

Singh, Simon. 1997. *Fermat's Last Theorem*. London: Fourth Estate

Snow, Charles P. 1993. *The Two Cultures*. Canto ed. Cambridge: Cambridge University Press.

Sobel, Dava. 1995. *Longitude*. New York: Walker

Sober, Elliott. 1975. *Simplicity*. Oxford: Clarendon Press.

Sokal, Alan. 1996a. "Transgressing the Boundaries: Toward a Transformative Hermeneutics of Quantum Gravity." *Social Text* 46-47:217-52.

Sokal, Alan. 1996b. "A Physicist Experiments With Cultural Studies." *Lingua Franca* 6(4):62-64.

Sokal, Alan. 2007. "Articles on the social text affair" [Web Page]. Accessed 25 Jan 2008. Available at <http://www.physics.nyu.edu/~as2/>.

- Sokal, Alan and Jean Bricmont. 1997. *Impostures Intellectuelles*. Paris: Editions Edile Jacob.
- Sokal, Alan and Jean Bricmont. 1998. *Fashionable Nonsense: Postmodern Intellectuals' Abuse of Science*. New York: Picador.
- Sokal, Alan and Jean Bricmont. 2001. "Science and Sociology of Science: Beyond War and Peace." Pp. 27-47 in *The One Culture*, Editors Jay A. Labinger and Harry Collins. Chicago: The University of Chicago Press.
- Sovacool, Benjamin. 2005. "Falsification and Demarcation in Astronomy and Cosmology." *Bulletin of Science, Technology & Society* 25(1):53-62.
- Spade, Paul V., editor. 1999. *The Cambridge Companion to Ockham*. Cambridge: Cambridge University Press.
- Spade, Paul V. 1999. "Occam's Nominalist Metaphysics: Some Main Themes." *The Cambridge Companion to Ockham*, Editor Paul V. Spade. Cambridge: Cambridge University Press.
- Spufford, Francis 2003: *Backroom Boys*. London: Faber
- Star, Susan L. and James R. Griesemer. 1989. "Institutional Ecology, 'Translations' and Boundary Objects: Amateurs and Professionals in Berkeley's Museum of Vertebrate Zoology, 1907-39." *Social Studies of Science* 19(3):387-420.
- Stewart, Ian. 1995. *Nature's Numbers*. London: Weidenfeld & Nicolson
- Stotz, Karola and Paul E. Griffiths. 2004. "Genes: Philosophical Analyses Put to the Test ." *History and Philosophy of the Life Sciences* 26(1):5-28.
- Stotz, Karola, Paul E. Griffiths, and Rob Knight. 2004. "How Biologists Conceptualize Genes: An Empirical Study." *Studies in the History and Philosophy of Science, Part C: Biological and Biomedical Sciences* 35:647-73.
- Strathern, Paul. 2000. *Mendeleev's Dream - The Quest for the Elements*. London: Hamish Hamilton
- Sturgis, Patrick and Nicholas Allum. 2004. "Science in Society: Re-Evaluating the Deficit Model of Public Attitudes." *Public Understanding of Science* 13:55-74.
- Tajfel, Henri. 1972. "Social Categorization, English Manuscript of 'La Categorisation Sociale'." *Introduction a La Psychologie Sociale, Vol. 1*, editor Serge Moscovici. Paris: Larousse.
- Tajfel, Henri. 1981. *Human Groups and Social Categories: Studies in Social Psychology*. Cambridge: Cambridge University Press.
- Tajfel, Henri. 1982. "Social Psychology of Intergroup Relations." *Annual Review of Psychology* 33(1-39).
- Tajfel, Henri and Turner J C. 1985. "The Social Identity Theory of Intergroup Behavior ." Pp. 7-24 in *Psychology of Intergroup Relations*, editors Worchel S and Austin W G. Chicago: Nelson-Hall.
- Tajfel, Henri and A L. Wilkes. 1963. "Classification and Quantitative Judgement." *British Journal of Social Psychology* 54:101-14.

- Theocharis, Theo. 2001. "A Physicist Experiments With Physical Studies." *Theory and Science* 2.
- Thorburn, W M. 1918. "The Myth of Occam's Razor." *Mind* 27(107):345-53.
- Tiles, Mary . 2005. "Technology, Science, and Inexact Knowledge: Bachelard's Non-Cartesian Epistemology." *Continental Philosophy of Science*, Editor Gary Gutting. Malden, MA: Blackwell.
- Turney, Jon. 1996. "To Know Science Is to Love It?" London: COPUS .
- Turney, Jon. 2001. "More Than Just Story Telling - Reflecting on Popular Science." *Science Communication in Theory and Practice*, Editors Susan M. Stocklmeyer, Michael M. Gore, and Chris Bryant. Dordrecht: Kluwer.
- Turney, Jon. 2007. "The Latest Boom in Popular Science Books." Pp. 81-92 in *Journalism, Science and Society*, editors Martin Bauer and Massimiano Bucchi. London: Routledge.
- Waldzus, Sven, Amélie Mummendey, Michael Wenzel, and Franziska Boettcher. 2004. "Of Bikers, Teachers and Germans: Groups' Diverging Views About Their Prototypicality." *British Journal of Social Psychology* 43(3):385-400.
- Walker, Alan, and Pat Shipman. 1996. *The Wisdom of Bones*. London: Weidenfeld & Nicolson
- Washer, Peter. 2004. "Representations of SARS in the British Newspapers." *Social Science and Medicine* 59:2561 – 2571.
- Washer, Peter. 2006. "Representations of Mad Cow Disease." *Social Science and Medicine* 62(2):457-66.
- Webb, Stephen. 2002. *Where Is Everybody?* New York: Praxis.
- Weinberg, Robert. 1998. *One Renegade Cell*. London: Weidenfeld & Nicolson.
- Weinberg, Steven. 1993. *Dreams of a Final Theory*. London: Hutchinson Radius.
- Weiner, Jonathan. 2000. *Time, Love, Memory*. London: Faber & Faber
- White, Michael. 2002. *Rivals*. London: Vintage
- Wills, Christopher. 1998. *Children of Prometheus*. London: Allen Lane.
- Wilson, Edward O. 1971. *The Insect Societies*. Cambridge, MA: Belknap Press of Harvard University Press.
- Wilson, Edward O. 1975. *Sociobiology: The New Synthesis*. Cambridge, MA: Belknap Press of Harvard University Press.
- Wilson, Edward O. 1978. *On Human Nature*. Cambridge, MA: Harvard University Press.
- Wilson, Edward O. 1994. *Naturalist*. London: Penguin.
- Wilson, Edward O. 1998. *Consilience*. London: Abacus.
- Wolpert, Lewis. 1992. *The Unnatural Nature of Science*. London: Faber and Faber.

- Wolpert, Lewis. 1999. *Malignant Sadness: the Anatomy of Depression*. London: Faber and Faber.
- Wright, Lawrence. 1997. *Twins: And what they tell us about who we are*. London: Weidenfeld & Nicolson
- Wynne, Brian. 1992. "Misunderstood Misunderstanding: Social Identities and Public Uptake of Science." *Public Understanding of Science* 1(281-304).
- Wynne, Brian. 1993. "Public Uptake of Science: a Case for Institutional Reflexivity." *Public Understanding of Science* 2(4):321-37.
- Yearley, Steven. 1988. "Settling Accounts: Action, Accounts and Sociological Explanation." *British Journal of Sociology* 39(4):578-99.
- Yearley, Steven. 2005. *Making Sense of Science*. London: Sage.

